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CLOSER RELATIONS BETWEEN TRUSTEES AND FACULTY.[1]

I VENTURE to speak upon the topic: 'Closer Relations between Trustees and Faculty' because I am in this respect hermaphroditic. I have seen service upon both college bodies and, moreover, have studied certain problems of public school administration which present many points of analogy. I speak, however, with only that half-knowledge which we of the east, unfamiliar with state-supported universities, bring to the important questions of this conference.

It is a common cry that teachers—whether in colleges or in schools—are underpaid; and the complaint (especially if one has been a school official) seems amply justified. The imperative need of our American college faculties, however, is not higher salaries; it is larger professional authority and more genuine freedom. Those attained, the wage question will take care of itself. It is true that teaching offers no such money prizes as does law or medicine; nevertheless, the average professor or schoolmaster is in many ways better situated than the average lawyer or physician. Despite this patent fact, the number of youth who deliberately prepare themselves to be teachers, by years of serious study, is comparatively small. Young men of power and ambition scorn what should be reckoned the noblest of professions, not because that profession

[1] Delivered at the Conference of Trustees of American Colleges and Universities, at the University of Illinois, October 17, 1905.

condemns them to poverty, but because it dooms them to a sort of servitude. The American lawyer or physician is subject only to the judgment of his peers—that is, to the well-established code of his profession. The American teacher, on the contrary, especially in the public schools, is not only subject to—he is often wholly at the mercy of—unsympathetic laymen.

This condition is inherent in the American system of education, and neither can nor should be wholly abrogated. The teacher serves the public (for even an *endowed* college is a public institution) and must rest, therefore, under some of a servant's disabilities. Yet, without impairing the proper powers of school or college trustees, it is possible, I believe, to give teachers—or rather to restore to them—so much of authority, dignity and independence as shall raise teaching to the professional status of the law—to a position, that is, where it will commend itself to the most ambitious and the best-trained youth.

The medieval universities, as you know, were preeminently nurseries and citadels of intellectual freedom and political democracy. They were 'essentially federated republics, the government of which pertained either to the whole body of the masters * * * or to the whole body of the students.' Moreover, 'what slight subordination did exist was, in the beginning, to the ecclesiastical and, later, to the civil power.' The American universities, also, from the frontier college of Harvard, in 1636, to the latest frontier (if there now is any such place) college of the plains—have been strongholds of intellectual freedom; but in their administration they have been profoundly subordinate, in the early days to the ecclesiastical, and later—directly or indirectly—to the civil power.

This subordination, under the stress of circumstances, has progressed until, as President Pritchett points out in a recent admirable address, the American university has become an autocracy, wholly foreign in spirit and plan to our political ideals and little short of amazing to those models of thoroughgoing democracy, the German universities. And this absolutism of the American university is not, as in the days of the scholastics, an autocracy of teachers and scholars; it is an autocracy of ecclesiastical or lay trustees. Whence has arisen this astonishing inversion? Why does the very fountain of our higher life present this paradox? Mainly, I think, because the European universities grew from within, while those of this country have been established from without. The old theocracy of New England, the younger democracies of her splendid daughters, created colleges to fit youth for service in church or commonwealth, and they placed over them men of notable authority. In the east, the hands of both church and state have been largely withdrawn; but in their place have appeared the dead or living hands of donors demanding that their gifts be safeguarded by stable and substantially irremovable trustees. College and public school funds are no less sacred than they are colossal; and those who administer them assume high legal as well as moral responsibility. But this large liability has been more than balanced by the gift of almost absolute powers—powers surpassing, perhaps, those of any other bodies. I do not know how it is here; but in Massachusetts the school boards are virtually despotic, far transcending in authority those sturdy democrats, their parent town meetings.

Excepting those strictly denominational, the balance of the extraordinary legal powers given to college trustees has gradually passed from the hands of the clergy into those of laymen chosen, as a rule, for their standing as financiers rather than as educators. From many aspects this has

been a salutary change; but there has followed from it one signal disadvantage—that of putting the trustees more and more out of touch with the faculties whose members they appoint. Although the reverend gentlemen of those antique college boards could scarcely have distinguished a government bond from a wildcat stock, they were usually scholars by inclination and teachers by profession, and their relations with their faculties were close and sympathetic; while the modern financier who, by skillful investing, secures every possible penny of income for his college, generally finds its educational problems quite outside his range, and sees, therefore, less and less occasion for meeting, or even knowing, that faculty over which, legally, his power is of life and death.

This change in personnel, however, is not alone responsible for the progressive alienation between trustees and faculty. That estrangement has come about, no less, through the rapid growth of college curriculums and in college attendance. When educational institutions were small and their courses of study undifferentiated, it was possible for trustees, even though not trained as teachers, to acquire an admirable education (so far as concerned their own college) through intimate relations with the faculty and personal supervision of their work. But with the enormous development in numbers and complexity, this old-fashioned contact between trustees and teachers has become impossible, and, at best, a trustee can now make himself familiar with only that department of the university which it is his duty (more honored in the breach than in the observance) to inspect. Therefore, the modern trustee has gradually withdrawn from the teaching side of the college to fix his attention upon those questions of revenue, housing and legislation which have multiplied even faster than the undergraduates.

But here again the size and complexity of the problem are appalling to men already overweighted with other responsibilities. These material questions, however, must be met and settled just as those on the educational side must be faced and solved. And both business and political experience have taught men of the world that the quickest and least troublesome way to solve administrative problems is to give as free a hand as possible to some man with brains, with tact, with power of initiative, of leadership, and of persuasion—with, in short, those peculiar abilities which distinguish the generals of our intricate twentieth century enterprises.

Hence has arisen the modern college president—a being as different from the awe-inspiring clergymen of the eighteenth century or from such men as Josiah Quincy (who was given the presidency of Harvard as a sort of haven for his declining years) as it is possible to imagine. The modern executives have had thrust upon them powers which give to their decrees the finality of an imperial ukase. They have assumed such sway, not from love of dominion, but because their task is so enormous that nothing short of practically plenary powers would permit of its being done at all. And it should be said to their honor that they have met the demands upon them as organizers and administrators so ably that, today, the leaders of the country are not, as formerly, the great statesmen and clergymen; they are these modern Cæsars—the heads of our principal colleges and universities.

These modern presidents have their cabinets in the board of trustees (if that board be small) or in an executive committee selected from it if the board be large; they have their staff in the several administrative officers, such as deans and registrars; they have their field officers in the heads of departments or courses; and the work

of the great machine, through committees, sub-committees, labor-saving devices and automatic methods of reporting, is as smooth-running (and sometimes, I fear, almost as impersonal) as a well-developed mercantile establishment. We have here a conspicuous example of the current tendency towards one-man power, towards that concentration of authority which makes, of course, for ease, rapidity and sureness of administration; but which, in politics, undermines manhood; in industrialism, destroys initiative; and in education tends to defeat the very object of teaching, which should be to develop and make the most of every man's individuality. If the goal of a college were the giving of mere instruction, nothing could be better than the present system of administration; but colleges should be fountains of true education, and the best part of education comes through the personal influence of the older governors and teachers upon adolescent, and therefore highly impressionable, youth.

Most modern colleges have expensive and excellent material plants utilized substantially to their full capacity. They possess, also, admirable executives who, as I have said, are used away beyond their limits of endurance. But those colleges have also other educational forces which are not availed of, in my opinion, to anything like their normal maximum. Those less used forces are: (1) The personal influence, as teachers and men (not as mere administrators) of the leaders of the faculty—an influence which should be exerted upon both students and trustees; (2) the personal influence, as men of power and broad human experience (not as mere money-holders) of the trustees—an influence which should extend to students as well as faculty; and (3) the perennial and unselfish loyalty of the alumni, together with the unique experience given to those graduates in gauging their collegiate training by the tests of life. The third force is beyond the scope of the present paper; but let it not be inferred, therefore, that I regard it as any less potent than the other two. Indeed, in the last analysis, the moral as well as the financial strength of a college must come from its own sons.

As has already been suggested, the complexity and autocracy of the American university have converted the strongest men of the faculty—the men, therefore, whose personal influence upon the students would be of the highest value—into subordinate administrators harassed with details of department maintenance and committee attendance. As a necessary result, the teaching is put largely into the hands of recently graduated youth, zealous but not always wise, untrained in the science and art of teaching, and quite incapable, of course, of giving to their classes the inspiration which comes from contact with men of wide experience. This throws the severest strain of the college upon the weakest part, and from it arises much of our educational ineffectiveness. Mere information, lesson-hearing, examinations, become paramount; scholarship and character are well-nigh forgotten, being impossible to register by even the most elaborate machinery.

The trustees, on the other hand—excepting those who constitute the president's cabinet—find less and less opportunity for usefulness in a machine so elaborate that any incursion into it, by those unfamiliar, may do infinite harm. Therefore most of them drift into the belief that their trust is discharged by attendance upon stated meetings and by, perhaps, an annual visit to that department which, nominally, is their especial care. Yet the personal influence upon the students of men like college trustees would be second only, in educational value, to that of the leading members of the faculty. I am not prepared to sug-

gest any plan by which the trustees can be brought into direct personal relations with the students; but I firmly believe that such a plan could be devised; and I know that nothing so vivifies a man of middle life and of large responsibilities, nothing so clears his brain and rejuvenates his heart, as comradeship with bubbling and eager undergraduates.

Whether or not trustees can broaden their powers and sweeten their responsibilities by thus meeting their students directly, it is clear that they can influence them indirectly by establishing closer relations with those young men's teachers. For their pupils' sakes and for their own advantage, the professors need the stimulus and the breadth of view which they would get from looking at the world through the eyes of such a man of affairs as the usual trustee; those trustees, on the other hand, need the insight into true education and into the difficulties of training youth which they would secure from intimate contact with the members of their faculty. The money conservatism of the trustee, hesitating to grant funds for new enterprises, needs to be enlightened by the vision which the teacher has of the demands and possibilities of higher education. *Per contra,* the academic conservatism of the scholar needs to be quickened by the hard world-experience of a man of more varied responsibilities. That purblind vision of the 'practical' man which exaggerates material success requires enlightenment through the opposite, but no less purblind, vision of the scholar which magnifies intellectual achievement. Each point of view is essential to the ends of true education, and unless each in authority can see and understand the other's outlook, the university will suffer and its youth will be defrauded of some of the best things in college.

At present—except for certain perfunctory visiting—almost the sole point of contact between trustees and faculty is their common sovereign, the president, who, as I have tried to show, has administrative duties and responsibilities beyond normal powers. Moreover, however conscientious he may be, his personal equation can not but enter into his interpretations—so to speak—between two bodies of which he alone is a common factor. It is essential to his leadership that he should have large powers over the teaching staff, but the opinions of the most perfect of administrators as to the individuals under his benevolent despotism should have the salutary check of others' close and unbiased observations.

In order, therefore, that there may be many instead of only one channel of understanding between trustees and faculty (as well as for the more subtle reasons suggested earlier), I would advocate most earnestly the creation in every board of trustees of a new standing committee. This committee should be most carefully chosen, and its duty should be to confer, at stated and frequent intervals, with a like standing committee of the faculty, selected freely by that body itself. And I would advise, further, that this conference committee be distinct, if possible, from that executive committee which I have called the president's cabinet, and that no legislation of any consequence should be passed by the executive committee or by the trustees as a whole without the concurrence of this joint committee. And—at least so far as relates to questions having any educational bearing—I would have it understood that the joint committee should *not* concur until the proposed action had been submitted to the faculty as a whole, had been debated, if so desired, before the standing committee and the executive committee sitting in joint session, and had been approved by at least a majority of the teaching staff.

Such a general plan as this (the details of which, needless to say, would differ with each college) could not fail, it seems to me, to increase the educational efficiency of a college to an extraordinary degree, by coordinating the views of those without and those within the daily routine of teaching; by establishing a clear understanding, in each body, of the other's problems; by relieving the executive of a substantial portion of his crushing load, through increasing the legislative and administrative responsibility of the faculty; and, not least, by making that faculty—without adding to its legal powers—a body coordinate with, instead of subordinate to, the board of trustees. Unless American college teachers can be assured by some such change as this that they are no longer to be looked upon as mere employees paid to do the bidding of men who, however courteous or however eminent, have not the faculty's professional knowledge of the complicated problems of education, our universities will suffer increasingly from a dearth of strong men and teaching will remain outside the pale of the really learned professions. As I said in the beginning, the problem is *not* one of wages; for no university can ever become rich enough to buy the independence of any man who is really worth the purchasing.

This plan of cooperation would not, however, except to a limited degree, bring the trustees as men into close contact with the faculty as men. And the plan which I offer towards that second aim is put forward with much greater diffidence. The scheme of a joint standing committee would be productive, I feel certain, of most happy results; but of my minor proposition I am not so sure. This second plan is to make every member of the board of trustees an administrative officer in that branch of the college work (so far as possible) which is most congenial to him, giving him no special individual powers over his assigned department, but increasing his responsibilities by making him—together with one or more of his colleagues—the direct and responsible channel of information between that department and the whole board of trustees. It is already customary in most colleges to create visiting committees with the duty of presenting annual reports; my suggestion would make substance out of what is now little more than shadow, by having it formally understood that in all matters relating to his department the trustee would be looked to for reliable information and responsible advice.

Difficulties, of course, stand thick in the way of such a project. Among them are the unwillingness of already busy trustees to accept further responsibilities, the danger of personal friction between the trustee and the department head, and the natural fear on the part of the teacher that 'administration' might spell itself to the trustee as mere officiousness. It seems to me, however, that a short acquaintance with the minutiæ of a college department would show the trustee that the professor's as well as his own time is far too valuable to be given to details of administration, and that college funds could in no way be made more productive than by giving the heads of departments such clerks and underlings as would release them from much killing drudgery. There is no greater extravagance than to permit an expensively trained man to do ten-dollar-a-week work. And that same short acquaintance would, I believe, so interest the trustee and so increase his respect for what is being done and what is still to do, that officiousness or meddling would become impossible.

These two plans, if found practicable and if developed in a spirit of enthusiasm,

would lead to many other points of helpful contact between trustees and faculty and would discover, I think, unsuspected avenues of mutual help. And by these or some like methods trustees and faculties must be brought more closely together unless we wish to see the growing alienation of the administrative and teaching staffs develop into a real and fatal breach. Separation involves mutual misunderstanding and that, even among educated men, leads, as in industrial enterprises, to arrogance on the part of the employer, to suspicion and dislike on the side of the employed. If cooperation seems imperative—as I think it does—to the solution of the problems of industrialism, how much more necessary is it if we are to solve the educational riddle. Cooperation would teach the trustees the antipodal difference between the problems of a university and those of a business corporation, and, at the same time, would show the faculty the importance of business methods and thorough organization. Cooperation would get things done without compelling our universities to take refuge in an autocracy which, harmful in itself, is breeding a race of youth who scorn the slow methods of democracy. It would develop trustees who actually, instead of fictitiously, comprehend and apprehend their trust; it would unite faculties which, under the strain of departmental complexity, are fast disintegrating; it would double the educational efficiency of our colleges; and, most important of all, it would make our universities, as they ought to be, supreme preservers, instead of conspicuous destroyers, of that genuine spirit of democracy which, more than schools, more than churches, more than any other human agency, has uplifted mankind and builded civilization.

JAMES P. MUNROE.

THE BIOLOGICAL LABORATORY OF THE BUREAU OF FISHERIES AT WOODS HOLE, MASS.: REPORT OF WORK FOR THE SUMMER OF 1905.

I. STAFF EQUIPMENT, ETC.

THE laboratory was in service for purposes of investigation during a period of somewhat over three months, commencing June 15. In addition to the crews of the vessels and the permanent force of the station, forty-three persons were directly or indirectly engaged in the furtherance of scientific research. Of this number, thirty are to be classed as investigators, their individual work being detailed below. Of these thirty, fifteen received a salary from the bureau, the remaining fifteen being unpaid. The staff of the laboratory consisted of a director, together with twenty-two others officially listed as 'temporary assistants'; and a librarian, clerk and janitor detailed from the office force at Washington. Of those classed as 'temporary assistants' six were occupied primarily in connection with the biological survey; five others in individual investigations conducted on behalf of the bureau; the remainder being engaged in miscellaneous work in laboratory, residence or field. Those who devoted the whole or part of their time to biological work requiring previous training have been included among the investigators.

The steam vessels *Fish Hawk, Phalarope* and *Blue Wing* were, as usual, in service during a large part of the season, and the much valued collection of biological works belonging to Brown University was again placed at the disposal of the laboratory.

Material for research was furnished by the dredging operations of the *Fish Hawk* and *Phalarope,* by the laboratory fish trap in Buzzards Bay, and by the large group of traps at Menemsha Bight, Marthas Vineyard, where a camp was established for

about six weeks. Special collecting trips for particular sorts of material were, of course, frequently made.

The plan was continued of holding informal weekly meetings devoted for the most part to reports upon researches in progress at the laboratory. Papers were presented by Messrs. Bigelow, Cushman, Gulick, Hargitt, Linton, McClendon, Stockard, Sullivan, Sumner and Thompson. These meetings were as a rule well attended by outsiders as well as by the investigators of this laboratory.

II. BIOLOGICAL SURVEY.

This was brought to such a point that a preliminary report upon the fauna and flora of the sea-bottom of this region might be profitably undertaken without further dredging work. It is hoped that the publication of such a report will be possible within a year or two.

The *Fish Hawk*, under command of Lieut. Franklin Swift, U.S.N., was engaged during a part of July in reestablishing the former coast survey tripods upon Marthas Vineyard and the Elizabeth Islands. Following this, a considerable portion of the bottom of Vineyard Sound, already dredged during the summer of 1903, was once more explored. The new dredging stations, being located by the aid of the coast survey signals, could be charted with far greater accuracy than had hitherto been possible. The extreme eastern portion of the sound, which had previously been left out of consideration, was likewise covered by the dredge. In all 101 'stations' were established by the *Fish Hawk* during a period of about six weeks. The dredging work of this vessel, together with the cataloguing of material thus obtained were under the supervision of Mr. L. J. Cole.

The *Phalarope*, under the direction of Mr. R. C. Osburn, worked in the shallower waters of Buzzards Bay, completing the entire eastern shore-line, from Cuttyhunk northward to Wareham.

Most of the material obtained by the dredge was identified in the laboratory by those in immediate charge of the work, with the voluntary cooperation of a number of systematists who were engaged in studies at the station. Thus the Foraminifera were determined by Mr. B. A. Cushman, who has likewise undertaken the identification of the sponges; the hydroids were referred to Professor C. W. Hargitt, the annelids to Dr. J. P. Moore, and the decapod crustaceans to Miss M. J. Rathbun. Considerable other material, however, still awaits disposal.

A camp was maintained at Menemsha Bight, Marthas Vineyard, for a period of about six weeks in July and August, three assistants at a time being detailed for this work. The numerous fish traps of this region were visited, and a daily record was kept of the species therein taken. Much material of value was likewise supplied to the laboratory for the use of various investigators, and several species of especial interest were taken, including a small specimen of the great blue shark (*Prionace glauca*), the only one recorded for this region within twenty-eight years. The torpedo (*Tetranarce occidentalis*) and the goose fish (*Lophius piscatorius*) were at one time rather common, and large specimens of the former species were kept on exhibition in the aquaria tanks.

An account of the collecting work at this station is never complete without mention of the labors of Mr. Vinal Edwards, whose well-directed search for fishes, fish parasites, plankton, etc., has made possible many of the most important contributions from the laboratory.

III. INDIVIDUAL INVESTIGATIONS.

The investigators were thirty in number,

representing eighteen educational and scientific institutions, ranging from Boston to the Pacific Coast, and from Florida to Montreal. Of these Harvard furnished eight, Columbia five, the University of Pennsylvania and the College of the City of New York two each.

The subjects of research may be somewhat arbitrarily classified as follows, allowing for the fact that certain investigators devoted their time to more than one subject:

Reactions to stimuli, animal behavior, etc.	9
Faunal and floral distribution (including survey work)	7
Taxonomy	6
Embryology	3
Ecology (exclusive of distribution)	2
General Physiology (exclusive of 'behavior')	2
Regeneration	2
Miscellaneous	6
	37

While much of this work is calculated to be of ultimate value in the solution of practical fisheries questions, it is true that little of it has in view immediate and obvious economic results. The most notable exception is a study of the potential food value of various unused or little-used marine organisms. This liberal attitude toward biological investigation is one of the traditions of the United States Fish Commission and its successor, the Bureau of Fisheries, which have never failed to recognize the vast indebtedness of industry to pure science.

In addition to those who have been listed as investigators, two painters of animal life, Messrs. Chas. R. Knight and S. F. Denton, made use of the facilities of the laboratory for a part of the season.

Robert P. Bigelow, Ph.D., instructor in biology, Massachusetts Institute of Technology: systematic study of the stomatopods collected by the steamer *Albatross* (continued).

Leon J. Cole, assistant in zoology, Harvard University: Biological survey of local waters. Mr. Cole had immediate supervision of the dredging work of the steamer *Fish Hawk,* and of the preservation and identification of material incidental thereto (salaried assistant).

Joseph A. Cushman, assistant curator, Boston Society of Natural History: Systematic study of the Foraminifera collected during the dredging operations of the steamers *Fish Hawk* and *Phalarope;* likewise collection of local marine ostracods for future identification.

Bradley M. Davis, Ph.D., assistant professor of botany, University of Chicago: Local marine flora. Dr. Davis continued his services in the capacity of botanist of the biological survey (salaried assistant).

Donald W. Davis, Harvard University: Dredging work of biological survey; also at Menemsha Camp (salaried assistant).

Pauline H. Dederer, lecturer in zoology, Barnard College, Columbia University: Studies upon the larval development of *Amaræcium.*

Irving A. Feld, Austin teaching fellow in zoology, Harvard University: (1) Further investigations of the food of certain fishes (principally *Raja erinacea*); (2) the food value of some hitherto unused or little used marine animals (fishes, crustacea and molluscs), tested by various cooking methods (*e. g.,* steam, under pressure); (3) experiments to determine the effect of concussions caused by gun-fire upon fishes (salaried assistant).

Alexander Forbes, A.M., graduate student, Harvard University: Experiments upon the attachment of oysters.

William K. Gregory, A.M., assistant at American Museum of Natural History: Comparative anatomy of local teleost fishes.

Addison Gulick, A.M., graduate student, Harvard University: (1) Experiments upon chemical sense of *Sycotypus canalic-*

ulatus, together with inquiry into the functions of the osphradium; (2) collection of material for future study of the glandular organs of molluscs.

Chas. W. Hargitt, Ph.D., professor of zoology, Syracuse University: Preparation of a synopsis of the Anthozoa of the Woods Hole region. This work will form one of the series of reports upon special groups of local fauna, issued at intervals by the Bureau of Fisheries (salaried assistant).

Trevor Kincaid, A.M., professor of zoology, University of Washington: Sporozoa of local marine invertebrates.

Edwin Linton, Ph.D., professor of biology, Washington and Jefferson College: The parasites of fishes, chiefly entozoa (salaried assistant).

J. F. McClendon, M.S., fellow in zoology, University of Pennsylvania: The early stages in the development of various parasitic copepods.

Hanford McCurdy, graduate student, Harvard University, instructor in biology, Manual Training School, Kansas City, Mo.: The reactions of *Pennaria tiarella* and *Eudendrium ramosum* to various stimuli; also experimental studies upon the nervous systems of local crabs.

A. G. Mayer, Ph.D., director research laboratory of Carnegie Institution, Dry Tortugas, Fla.: Experimental studies upon medusae.

J. Percy Moore, Ph.D., instructor in zoology, University of Pennsylvania: Synopsis of local annelids (for the bureau) (salaried assistant).

Chas. V. Morrill, graduate student, Columbia University: Dredging work of biological survey, likewise experiments upon regeneration of fishes (salaried assistant).

Max Morse, graduate student, Columbia University: (1) Dredging work, etc., (2) experiments upon autotomy of 'head' in *Tubularia crocea* (salaried assistant).

Raymond C. Osburn, instructor in zoology, New York High School of Commerce; graduate student, Columbia University: Biological survey of local waters. Mr. Osburn had direct supervision of the dredging work of the steamer *Phalarope* (salaried assistant).

A. S. Pearse, A.M., graduate student, Harvard University: The reactions of *Tubularia crocea* to various stimuli.

Mary J. Rathbun, assistant curator U. S. National Museum: Studies of habits of local decapod crustacea.

Carl D. Sawyer, medical student, McGill University: Studies of fish parasites (assisting Professor Linton) (salaried assistant).

G. G. Scott, A.M., tutor in anatomy and physiology, College of the City of New York: (1) Morphology of the tracheal system of a dragon-fly nymph; (2) relation of the central nervous system to regeneration in *Fundulus heteroclitus* (salaried assistant).

A. B. Seymour, Cambridge, Mass.: Studies of marine algæ of Woods Hole.

Chas. R. Stockard, M.S., graduate student, Columbia University: The effects of lithium chloride upon the development of the egg of *Fundulus heteroclitus*.

Michael X. Sullivan, Ph.D., instructor in chemical physiology, Brown University: (1) The physiology of the digestive tract in certain fishes; (2) the rectal gland in certain fishes (salaried assistant).

Francis B. Sumner, Ph.D., instructor in zoology, College of the City of New York; director of the laboratory: (1) Biological survey of local waters; (2) the physiological effects of changes in water density and salinity upon fishes. (Last shortly to be issued by the bureau.)

Millett T. Thompson, Ph.D., instructor in zoology, Collegiate Department, Clark University: The decorating instinct of

spider crabs (*Libinia emarginata* and *L. dubia*).

Herbert E. Walter, A.M., assistant in comparative anatomy, Harvard University: (1) Reactions of a triclad worm (*Bdelloura candida*) to light; (2) statistical study of the variability of the gastropod *Urosalpinx cinerea* from various points in the vicinity of Woods Hole.

FRANCIS B. SUMNER.

CONVENTION OF THE ASSOCIATION OF AMERICAN AGRICULTURAL COLLEGES AND EXPERIMENT STATIONS.

THE association met at Washington for its nineteenth annual convention, November 14–16, 1905. Being preceded and followed by a number of other conventions, among which were those of the Associations of State Universities, of Farmers' Institute Workers, of Horticultural Inspectors and of Official Agricultural Chemists, an unusually large number of persons interested in industrial education and agricultural experimentation was brought together in Washington, and this tended to make the convention of agricultural colleges and experiment stations one of the largest ever held. The association was addressed by the Hon. James Wilson, secretary of agriculture, who declared his deep interest in the work carried on by the institutions represented in the association, and indorsed the movement to secure an increase in the appropriation for the experiment stations, to which he pledged his support.

The annual address of the president of the association, delivered by Dr. E. B. Voorhees, director of the New Jersey experiment stations, related in the main to some of the duties and responsibilities of the agricultural colleges and experiment stations. Dr. Voorhees held the colleges primarily responsible for the character of work done by the experiment stations, because the working staff is the first determining factor in station work, and the colleges must be depended on to furnish the fundamental training for this work. He urged the need of more research work on the part of the stations, and declared that the present limitations were largely due to the insufficient supply of broadly trained men competent to conduct highly scientific investigations in agriculture.

A memorial address upon the life and services of President Henry H. Goodell, of the Massachusetts Agricultural College, and for many years a prominent worker in the association, was delivered by President W. E. Stone. President Goodell's death occurred last spring.

There were the usual reports of officers of the association and of standing committees. The executive committee reported that it had succeeded during the year in securing modification of the orders of the War Department with reference to military instruction in the land-grant colleges. The same committee suggested a reorganization of the standing committees of the association, which was taken up later and was one of the important actions of the association. Four standing committees were provided for, viz., (1) instruction in agriculture, (2) graduate study, (3) extension work and (4) experiment station organization and policy. The committees consist of six members each, and there is provision for a gradual rotation in the membership, the terms of only two members expiring each year.

Dr. A. C. True presented the report of the committee on indexing agricultural literature, describing the progress which has been made by the library of the Department of Agriculture in indexing scientific periodicals relating to agricultural investigations. The index is printed on cards. This undertaking has grown out of the efforts of the committee. Dr. True also presented the report of the committee on

methods of teaching agriculture, outlining the future work of the committee and mentioning the proposed extension of the studies in matters relating to agricultural education in the Office of Experiment Stations, an appropriation for which has been recommended to congress.

The committee on graduate study reported, through Professor L. H. Bailey, that arrangements had been made for holding the second session of the school of graduate study at the University of Illinois during the coming summer. The association took more definite action in fixing the relations of this school, adopting it as a regular part of its work and assuming responsibility for its maintenance and success. Contributions from the various agricultural colleges have been pledged toward the support of the school.

There was considerable discussion of the bills before the last congress providing increased endowment for the agricultural experiment stations and for the establishment of mining schools, and the association instructed its executive committee to lend its aid toward securing the passage of these bills, and especially that providing additional funds for the experiment stations. The committee was also instructed to secure if possible the establishment of a department of agricultural education in the National Educational Association.

SECTION ON COLLEGE WORK AND ADMINISTRATION.

The general theme for discussion before this section was 'The Field and Functions of the Land-Grant Colleges,' which was considered under the heads—curriculum, discipline and environment. The program included papers upon 'A Minimum General Culture Requirement,' by President A. B. Storms; 'Relative Amounts of Pure and Applied Science,' by President J. M. Hamilton; 'Courses in Agriculture, Horticulture and Allied Subjects,' by Professor F. W. Rane; "What is a 'Liberal and Practical Education' for an Engineer?" by Dr. H. W. Tyler; 'A Degree Course in Home Economics,' by President W. E. Stone; 'What Ought a Degree Course in Home Economics to Include?' by President J. L. Snyder; 'Student Control,' by President W. O. Thompson; and the relations of the land-grant colleges to—(1) the state universities, by President W. J. Kerr, (2) the normal schools, by President K. C. Babcock, (3) the public schools, by Dr. A. C. True, and (4) the farmers, by Professor John Hamilton.

Throughout the discussion in this section there was apparent unanimity of opinion that the land-grant colleges should offer courses equal in educational value to those of other institutions granting degrees, that industrial subjects properly taught have a high educational value, and that the college courses should not be narrow or severely technical. There was also recognition of the fact that the function of the land-grant college includes more than the conducting of degree courses. These colleges should lend their aid, through cooperation with other established institutions and in other ways, to the establishment and development of efficient secondary and elementary courses in agriculture and the mechanic arts in the public schools or in special schools, supplementing their four-year courses with short courses and other forms of extension work until the work of the secondary and elementary schools is well established. They should also continue, so far as their resources will allow, the extension work intended to reach adult farmers and others unable to attend school.

SECTION ON EXPERIMENT STATION WORK.

There were two main themes considered by this section—a series of papers upon 'Soil Investigations,' and a general discussion upon the subject 'How Much Demon-

stration Work and What Kind should the Experiment Station undertake?' Under the first subject Dr. C. G. Hopkins presented a paper on 'Soil Fertility in Relation to Permanent Agriculture,' in which he outlined the method followed in Illinois in studying the problems of fertility and the fertilizer requirements, summarizing the results obtained and introduced into practise. Dr. A. M. Peter presented 'Some Results of an Old Method for Determining Available Plant Food in Soils,' giving the results upon virgin soil, old field soil and subsoil. He pointed out the weaknesses of the method and recommended a modified method, for which he gave a series of results. Director C. E. Thorne, in a paper on 'Soil Investigation,' pointed out the necessity of supplementing chemical analysis and pot experiments with carefully conducted field experiments, and also of giving more attention to the biological processes of the soil; and Dr. H. J. Wheeler described some of the lines of soil investigation in progress at the Rhode Island Experiment Station, making suggestions regarding profitable lines of research to be undertaken in this field.

The discussion upon the subject of demonstration work developed some difference of opinion as to the advisibility of the experiment stations undertaking such work, although its usefulness was freely acknowledged. A clear distinction was drawn between experimental and demonstration work, and the general opinion was advanced that the demonstration work was more strictly an education phase which might very properly be undertaken by other agencies, leaving the stations free to confine their efforts quite largely to experimentation and research. Considerable demonstration work is now carried on by the experiment stations, and this work is a necessary supplement of the present system of investigation and dissemination.

The association received an invitation to hold its next meeting in California, prior to or following the meeting of the National Educational Association. Considerable interest was manifested in this matter, an objection to the plan being that it might conflict with the proposed graduate school if held during July. The matter was left with the executive committee.

In the election of officers President M. H. Buckham, of Vermont, was chosen president; President C. C. Thach, of Alabama, Dr. E. H. Jenkins, of Connecticut, and Presidents J. H. Worst, of North Dakota, B. I. Wheeler, of California, and Luther Foster, of New Mexico, vice-presidents. Director J. L. Hills, of Vermont, was reelected secretary-treasurer; Dr. A. C. True, of the Department of Agriculture, bibliographer; and Dr. H. C. White, of Georgia, President J. L. Snyder, of Michigan, Dr. W. H. Jordan, of New York, Director C. F. Curtiss, of Iowa, and Director L. H. Bailey, of New York, members of the executive committee. President C. R. Van Hise, of Wisconsin, was chosen chairman, and President H. C. Price, of Ohio, secretary, of the section on college work and administration; and Director B. C. Buffum, of Wyoming, chairman, and Director M. A. Scovell, of Kentucky, secretary, of the section on experiment station work.

E. W. ALLEN.

WASHINGTON, D. C.

SCIENTIFIC BOOKS.

JORDAN'S GUIDE TO THE STUDY OF FISHES.[1]

IN 1880, ten years after completing his great 'Catalogue of the Fishes in the British Museum,' Guenther published his 'Introduction to the Study of Fishes,' a book of 720 pages which gives 'in a concise form an ac-

[1] Jordan, David Starr, 'A Guide to the Study of Fishes,' 2 Vols., small quarto, pp. xxvi + 624 and xxii + 599. 2 colored frontispieces and 899 illustrations. New York, Henry Holt & Co., 1905. $12.00 net.

count of the principal facts relating to the structure, classification, and life history of Fishes' and is 'intended to meet the requirements of those who are desirous of studying the elements of Ichthyology; to serve as a book of reference to zoologists generally; and, finally, to supply those who, like travelers, have frequent opportunity of observing fishes, with a ready means of obtaining information' (preface). Guenther's 'Introduction' has continued the only work of its sort. Indeed, of comprehensive works on fishes, there have appeared, aside from popular natural histories, only Dean's excellent 'Fishes, Living and Fossil' (1895), a strictly morphological work, and the seventh volume of the 'Cambridge Natural History' (1904), containing Bridge and Boulenger's admirable 620 pages on fishes. The latter treatise is almost as extensive as Guenther's 'Introduction' and will largely supersede it.

In 1900 President Jordan (with Dr. Evermann) completed his monumental but strictly systematic 'Fishes of Middle and North America.' In 1902 he (again with Dr. Evermann) published the popular 'American Food and Game Fishes.'

The present work, the first large work on fishes to be independently undertaken by the writer, is of nearly twice the extent of Guenther's 'Introduction' or of the 'Cambridge Natural History' and like them "treats of the fish from all the varied points of view. * * * The writer has hoped to make a book valuable to technical students, interesting to anglers and nature lovers, and instructive to all who open its pages" (preface). In scope and in the circumstances leading up to its writing Jordan's book suggests comparison with Guenther's. Like its predecessors, it falls into two parts. The first twenty-four chapters (459 pages) form an introductory or general treatise on fishes while the remainder of the work deals descriptively with the various subdivisions of the group. The following chapter headings indicate the scope of the general part: I., The Life of the Fish; II., The Exterior of the Fish; III., The Dissection of the Fish; IV., Skeleton; V., Morphology of the Fins; VI., Organs of Respiration; VII., Nervous System; VIII., Organs of Sense; IX., Organs of Reproduction; X., Embryology and Growth; XI., Instincts, Habits, and Adaptations; XII., Adaptations; XIII., Colors; XIV., Geographical Distribution; XV., Isthmus Barriers Separating Fish Faunas; XVI., Dispersion of Fresh-water Fishes; XVII., Dispersion of Fresh-water Fishes, continued; XVIII., Fishes as Food for Man; XIX., Diseases of Fishes; XX., Mythology of Fishes; XXI., Classification; XXII., History of Ichthyology; XXIII., The Collection of Fishes; XXIV., The Evolution of Fishes (paleontological).

The author's interests as a taxonomist and student of geographical distribution seem to be reflected in his allotment of space. Thus in chapters I. to V. and XII. and XIII., about 134 pages, or nearly one third of the general portion, are devoted chiefly to the external morphology and the skeleton, the structures most used by the systematist. Geographical distribution occupies 83 pages and the history of ichthyology 42. The subjects mentioned thus take up more than one half of the introductory part of the book. On the other hand, respiration is dismissed with 18 pages, the nervous system and sense organs with 15 and the reproductive organs with 7, while five lines are given to the kidneys and they are not referred to in the index. The 'Cambridge Natural History,' with but half the space of the present work, gives 36 pages to respiration, 30 to the nervous system and sense organs and 24 to the urinogenital organs. While it is true that external features and particularly external adaptive features are of peculiar interest to the general reader, yet much that President Jordan has included on the skeleton and external morphology is not treated from the point of view of adaptation and is probably of less general interest than much that might have been added on internal structures.

In writing that the book is meant for technical students, anglers and nature lovers, the author clearly does not wish us to understand that all parts of it have been so written as to be useful to each of these classes of readers, but

rather that each will find in it much of interest and value. In truth the style is very unequal. The first three or four chapters and chapter sixteen, as well as many other portions, are models of clearness and simplicity and are of use to the general reader as well as to the specialist, while portions of the chapter on the morphology of the fins and of other chapters will hardly attract the nature lover. This inequality of matter arises largely from the fact that while a part has been prepared especially for this book and with reference to all the classes of readers mentioned, much of it has been quoted from more or less technical sources. Thus much of the chapter on the morphology of the fins is quoted from technical papers of Ryder, Kerr and Gill. The chapters on geographical distribution and on the history of ichthyology are, with little modification, reprinted from earlier papers of the author, some of them published in this journal. The chapter on color is from a paper of the author in the *American Naturalist* and that on collecting from a paper in the *Popular Science Monthly*. Most of the chapter on diseases is quoted from American authors, but Hofer's valuable 'Handbuch' is ignored.

So on the first page we read:

> If we would understand a fish, we must first go and catch one. This is not very hard to do, for there are plenty of them in the little rushing brook or among the lilies of the pond. Let us take a small hook, put on it an angle worm—no need to seek an elaborate artificial fly—and we will go to the old 'swimming hole' or the deep eddy at the root of the old stump where the stream has gnawed away the bank in changing its course.

This may savor of the nature-study leaflet but is clear withal and meaty. On the other hand, we have this quotation from Gill:

> The two elements of the arch, named by Parker, in *Lepidosiren*, 'supra-clavicle' (scapula) and 'clavicle' (ectocoracoid) seem to be comparable together, and as a whole, with the single element carrying the humerus and pectoral fin in the Crossopterygians (*Polypterus* and *Calomoichthys*) and other fishes, and therefore not identical respectively with the 'supraclavicle' and 'clavicle' (except in part) recognized by him in other fishes. (I., p. 89.)

Would not the book have been more readable for all had the author adhered to the plan with which he seems to have set out, that of presenting the whole subject in non-technical form and in his own clear and forceful language?

An examination of the general part of the book as to its scientific quality shows the same unevenness as in style and in the distribution of space. The chapters dealing with the internal organs (exclusive of the skeleton) omit much. Brüning's fundamental work on the physiology of the circulation of fishes has not been made use of. The account of the nervous system (chapter VII.) does not mention the thalamencephalon as one of the subdivisions of the brain, does not call attention to the distinctive features of the brain which result from the presence of the nervus lateralis and does not mention the lateralis. The account of the nervous system is then that of the elementary text-book of comparative anatomy of ten years ago, and includes nothing of the important and accessible recent work of Herrick, Johnston and other Americans.

The account of the eye does not discuss its most striking peculiarities worked out ten years ago by Beer,[2] and perpetuates an old error of Plateau, found in nearly all text-books, in the statement that the cornea is 'little convex,' though Beer showed that this is not the case.

One is left with a hazy impression of the sense of hearing in fishes after reading the contradictory statements on pages 115, 121 and 122; and this in spite of references to Parker's recent work. The sense of taste is dismissed with a paragraph and Herrick's important and most interesting work is not mentioned, nor are his results utilized.

A good deal of space is given to the subject of color, and yet the reader is likely to be left with an indefinite and in some respects erroneous notion of the cause of colors and the

[2] Beer, Th., 'Die Accommodation des Fischauges,' *Pflüger's Archiv*, LVIII., 523–650.

mechanism of color changes in fishes. We are told (I., p. 8) that changes in the position of the scales produce color changes; 'when they rise a little the color seems to change.' 'Fine lines and concentric striæ on the scales' are said to produce a bluish luster, although there is 'no real blue pigment' under them (I., p. 9). Again (I., p. 226) 'certain metallic shades, silvery blue or iridescent, are produced, not by actual pigment, but, as among insects, by the deflection of light from the polished skin or the striated surfaces of the scales.' And yet there are several references to blue pigment (I., pp. 8, 9, 129, 155). Pouchet[3] showed more than thirty years ago that the blue colors of fishes were due, not to the structure of the scales, but to a layer of cells containing guanin crystals, and to these cells he gave the name of iridocytes. Cunningham and McMunn,[4] though differing from Pouchet in details, reached the same conclusion. The pigments of fishes are lipochromes (red, orange, yellow) and melanin (black), not blue. All this is accurately and fully set forth in the 'Cambridge Natural History.' Moreover, Camichel and Mandaul[5] have recently made the general statement that in vertebrates all blue skins contain no pigment but black, and all green skins no pigment but black and yellow.

These are, to be sure, in a sense errors of omission, but there are other errors not of this sort. Thus we are told (I., p. 91) that the labyrinthine apparatus of *Anabas scandens* is a 'contrivance for holding water above the gills,' though Zograff[6] showed the apparatus to be for the purpose of holding air for respiration. Here again the 'Cambridge Natural History' would have furnished accurate information. On page 107 arterial bulb and conus arteriosus are used as synonyms. One of the strangest errors is in the statement (I., p. 118) that in the deep sea form *Ipnops* 'the eyes are spread out to cover the whole upper surface of the head, being modified as luminous organs.' This was Moseley's original opinion, but, as he states in his final paper,[7] it arose from the fact that among the sections of *Ipnops* given him for examination there was an unlabeled slide 'containing a section of the retina of some ordinary teleostean fish.' His final examination showed him that the 'peculiar organs have in reality no connection with organs of vision. The eyes as well as the optic nerves are entirely aborted in the fish.'

These instances from the general part of the book show that it can not be uniformly depended upon to present a full, accurate and modern account. On the other hand, the chapters on distribution, classification and the history of ichthyology do not appear to err in any of these directions. These subjects are treated with so great fullness and knowledge that nothing equal to them is to be found elsewhere. The chapters on distribution have the rare merit of relating the distribution to the topography and recent geological history of the regions discussed and are to be commended for their wise conservatism. The chapters dealing with the external structural features and that on paleontological history, though less technical, are usually clear and well suited to various classes of readers.

The descriptive portion of the book (somewhat more than one half) follows the method long familiar to American zoologists in Packard's 'Guide to the Study of Insects.' The salient characters of each family or larger group are given and are followed by a running and often gossipy comment on its commoner or more interesting representatives. There are no keys or specific descriptions, so that for determining species President Jordan's other books should be used. There is much difference of opinion among naturalists as to the value of this mode of treatment. While it affords much of interest to the technical student, it commonly fails him when he is in search of information about particular species.

[3] Pouchet, G., *Comptes Rendus*, LXXIV., 1341–1343 (1872).

[4] Cunningham and McMunn, *Phil. Trans. Roy. Soc. Lond.*, Vol. 184B, p. 765.

[5] Camichel and Mandaul, *Comptes Rendus*, CIII., 826 (1901).

[6] Zograff, *Quar. Jour. Micr. Sci.*, XXVIII., 501 (1888).

[7] Moseley, 'Challenger Reports,' XXII., 269.

On the other hand, for the non-technical student it makes a readable account, but makes it necessary for him to search long if he desires to place his specimen. Whatever one may think of the method, President Jordan has used it with skill, and the result is an account probably as attractive as such an account can well be made. Though it does not ignore foreign fishes, it is of particular interest to Americans, since it properly gives preference to American forms. The author has given us the cream of his long experience and wide knowledge of American fishes and has made it full and attractive. There is no other account of the sort for American readers. He has wisely treated the fossil fishes in their proper sequence along with living fishes. Many systematists will not agree with President Jordan concerning the general scheme of classification adopted and in minor details, but that is unavoidable and immaterial in a book of this kind.

The book is profusely illustrated with halftones of uniform excellence and with two striking colored plates. Ichthyologists should be especially grateful to Dr. Jordan for the admirable portraits of their colleagues, past and present. There is a valuable index, but the intentional omission of all but a very few references to the literature is to be regretted. Such selected foot-note references as are to be found in the 'Cambridge Natural History' occupy but little space and are of distinct value, even to the expert.

The publishers have given us a sightly page with large clear type and serviceable binding. The proof-reader has been sometimes at fault, though fortunately not often in the technical terms. There may be noted only 'construction' for contraction (p. 28); 'mallets' for mullets (p. 32); 'himera' for Chimera (p. 43); 'neutral' for neural (p. 56); 'ethnoid' for ethmoid (p. 113); 'Acancocephala' for Acanthocephala (p. 344). Other errors will be readily detected by the reader. In his chapter on mythology President Jordan has, apparently by the omission of a decimal point, added a new myth in making the 'sea-serpent' *Regalecus* 225 feet long (p. 361), but the measurement is correctly given in another place.

The book as a whole impresses the reader as the work of a busy man. Where the author has wandered from the narrower field of systematic ichthyology, with its attendant problems of distribution and external morphology, he has sometimes fallen into vagueness or error. Where he has depended on compilation he appears usually to have used only the more accessible and older English compendious sources and has thus helped to a longer currency some errors that others had corrected. Where, on the other hand, he has traversed his own familiar ground he has supplied a real need and supplied it admirably. In view of the great merits of his work, as he has himself said of the work of another, we may well 'pass by its faults with the leniency which we may hope that posterity may bestow on our own.'

JACOB REIGHARD.

Lectures on the Calculus of Variations. By OSKAR BOLZA. (The University of Chicago Decennial Publications, Second Series, Volume XIV.) Chicago, The University of Chicago Press. 1904. 8vo. Pp. xv + 271.

It is to be the function of the present generation of mathematical scholars in America to set a standard in mathematical memoirs and treatises which has been almost totally lacking in this country. The mathematical books which have heretofore appeared in America have been with few exceptions of text-book rank; it would be a generous estimate which would allow the fingers of both hands upon which to count books which would not be within the range of college students in undergraduate classes among the total mathematical production of the United States. There are several books in English, published in England, which are available; but the English school of mathematics has labored under such a handicap of lax reasoning and so-called 'general' statement that many of these can not be used with safety by one who is not already a master of the subject treated.

It is, therefore, a very notable occurrence when a mathematical work of really advanced

grade appears in America. Its standards of rigor, its terminologies, even its styles of typography, are destined to leave a relatively lasting influence upon future treatises of the same class. Such an influential book is Bolza's 'Lectures on the Calculus of Variations.' It will appear from what follows that this influence is to be great in many a subject besides the 'Calculus of Variations,' and that the quality of its influence is excellent in practically every particular.

The calculus of variations is one of the very first of the old (or formal) developments of the infinitesimal calculus; it is one of the newest conquests of the modern (or critical) school. The history of the older calculus of variations is almost trite from reiteration; to select among many, the works of Moigno-Lindelöf, Diegner, Todhunter, Caroll, Jordan and (more recently and more perfectly) Pascal, have made known the achievements of the old school from Newton to Jacobi to mathematicians throughout the world. The modern theory—the critical revision of the older theory—has appeared so recently that its existence is still unknown to many scientific men. Since the book under discussion is practically the first exponent of this newer work which is reasonably easy to follow, it seems fitting to state here the characteristic differences between the older theory and the new.

The essential problem may be stated here as follows: Given an integral of the form

$$I = \int_{x_0}^{x_1} f(x, y, y')dx,$$

where

$$y' = \frac{dy}{dx}$$

that integral will take on a fixed numerical value whenever a curve C is given by an equation of the form $y = \phi(x)$. It is proposed to find that curve C for which this numerical value is at a minimum (or maximum). The *finesse* of the whole modern theory, and the exquisite exactness of the present treatment, lies in so refining this crude statement that an accurate solution of the problem becomes possible. Thus the first question which arises is the following: Taking a supposed solution $y = \phi(x)$, are we to presume that the corresponding integral value is less than (or greater than) the integral value which corresponds to absolutely *any* other curve?—or to those which lie reasonably near to the supposed solution?—or to those which lie reasonably near and also have reasonably small difference in inclination? Are the 'comparison' curves to be reasonably continuous and have a respectable number of derivatives, or are we to admit all kinds of outlandish curves to our considerations—curves with corners—curves with no tangents—curves with no radius of curvature—curves which misbehave themselves in the variety of ways which are now known to be possible? If not, then in what sense may we say that the supposed solution really gives the integral a smaller value than any other curve near it? These questions are fundamentally important; their consideration gives to the modern theory an exactness which the older theory lacked completely; it is this attitude which renders Bolza's book a modern work.

The results of the modern theory are very satisfying in their evident accuracy, and they are none the less satisfactory in the simplicity of the conclusions reached. From a practical standpoint, it may be said that any curve which is a true solution of the problem must satisfy the very same conditions which were derived in the older books, viz., (1) the condition discovered by Euler (sometimes known as Lagrange's condition), (2) that discovered by Legendre, (3) that discovered by Jacobi. In so far there is no change, save for a greater accuracy in statement and a very material advance in the exactness of the proofs. These considerations practically exhaust the first two chapters of the book, and their presentation here can not fail to arouse lively enthusiasm in the intelligent reader on account of irresistible force of the precise reasoning and the consequent indisputable truth of the conclusions.

A curve may satisfy the preceding conditions, however, and still fail to render the integral a minimum (or maximum). This fact was discovered by Weierstrass, who succeeded

in setting up a *fourth* necessary condition. These statements apply, however, only when we admit as 'comparison curves' curves which, although they lie reasonably near the supposed solution, differ greatly from the supposed solution in inclination. For some physical applications it is evident from the nature of the problem that such comparison curves may be neglected. For example, in the famous problem of the 'Surface of Rotation of Least Resistance to a Moving Fluid,' which is due to Newton, there exist no solutions whatever in the sense just explained. That is to say, there exists no surface of revolution whatever which does not offer more resistance than some other surface of rotation, under the Newtonian assumptions regarding resistance. The possibility of such a result was entirely overlooked even within the lifetime of scientists now living, it being assumed as an *a priori* fact that some solution surely existed in this and many other similar problems. On the other hand, it is probable that any physicist would hold that the extreme *wavyness*—the krinklyness, to use a term invented by Professor Moore—which the surfaces of less resistance than the formal 'solution' must have, is really an objection to their consideration from a physicist's standpoint, in that the saw-like edges of the surface would cause a local eruption in the moving fluid which was not contemplated when the original assumptions regarding the nature and amount of the resistance were made. To meet such reasonable objection, a distinction is made between *strong* minimizing curves, *i. e.*, curves which render the integral of the problem under consideration absolutely *less* than *any* other curve *whatever* in their neighborhood, and, on the other hand, *weak* minimizing curves, *i. e.*, curves which render the integral less than any other curve which lies in the neighborhood and whose slope differs only slightly from that of the 'solution.' To clinch the point it may be stated that while there exists no solution of Newton's famous problem, there are 'weak' solutions, and these are precisely the stock solutions of the older books; the difference is that the newer theory demonstrates that there are curves which give a less resistance than the stock solution, whereas the older books would have led one to believe that such was not the case.

While this is merely a typical example, it illustrates in an essential manner the tendencies of the modern thought. The development, not of this example, but of the general theory of Weierstrass's condition and that of the *sufficient* conditions both for 'weak' and for 'strong' minimizing (or maximizing) curves forms the subject matter of the third chapter. It may be that such a precise theory is too far advanced for really practical application, but the writer is among those who believe that mechanics, physics and a few other sciences are upon the eve of the same reformation which has characterized the progress of mathematics during the past century, by which mathematics has become more really an 'exact science.' The fact that errors in conception and errors in proof have been made in that science which has been misnamed 'exact,' and that these errors have been at least partially removed through impartial and accurate investigation may be surprising to many who are not specialists in mathematics, but it would seem to suggest at the same time the possibility of similar investigation in sciences which have hitherto been classed as 'inexact,' in that it demonstrates that the difference is, after all, not so fundamental. Especially when the subject treated is as closely allied to mechanics and physics as is the calculus of variations, the possibility of rendering those subjects just as rigorously exact in certain chapters as are a few of the present branches of mathematics begins to appear less visionary. Possibly a confession in such a general scientific organ as is SCIENCE, that there are portions of mathematics—notably the theory of curves and surfaces—which are still open to fundamental criticism and objection, will tend to further the impression that there is no radical characteristic difference in exactness between mathematics and certain other sciences.

The next chapters will be of especial interest chiefly to those who are specialists in mathematics. Suffice it to say here that they treat other less specialized cases than the one

fundamental case mentioned above, and that they introduce a slightly more general form of notation, which is necessary in the last analysis for the solution of certain problems. An adequate account of these chapters can be omitted here with more grace since the writer has shown his appreciation of them in a manner more befitting their real importance in a review of a more technical character in the *Bulletin of the American Mathematical Society* (November, 1905), which will be accessible to a reader whose interest is professional.

It would be misleading to leave an impression that Bolza's book is elementary or that it can be read without special mathematical training. It is highly technical even in comparison with most of the extant *English* works on mathematics. Its standards are not higher than they should be, however; they are unusual in a book printed in English; they are not higher than the average standard of the classical foreign mathematical treatises; they *are* beyond question a step in the right direction.

This book is not primarily a text-book; it will not share the speedy oblivion of that class; and there are many evidences that this and a few other extremely recent mathematical books published in America are merely the first of a considerable volume of mathematical productions of like grade, which, if the average standards remain equal to those set by this book, will indeed deserve to be called an American mathematical literature.

E. R. HEDRICK.

COLUMBIA, MO.,
December 1, 1905.

SOCIETIES AND ACADEMIES.

THE WASHINGTON ACADEMY OF SCIENCES.

AT a meeting of the academy on November 28, Professor Wilhelm Ostwald, of Leipzig, who is now lecturing at Harvard University, gave an interesting address upon the proposed 'Universal Auxiliary Language.' The substance of his address was as follows:

In our time, when international congresses of all kinds, scientific, commercial, political, etc., are gathering almost every day, the necessity of a general means of mutual understanding is felt more keenly than at any former time. Small wonder therefore, that at one of these congresses, that of philosophy, held at Paris, 1900, the question was earnestly discussed, whether it was possible to overcome this insufferable obstacle to the common work of mankind. As a result, an international committee was elected to consider the matter and to propagate the general idea. This committee came to the conclusion that the selection or creation of a general auxiliary language was necessary, which should not supplant the existing languages, but should be learned by everybody besides his native speech for the purpose of international intercourse. This language must satisfy the following three conditions:

1. It must fulfill the needs of ordinary intercourse of social life, of commercial communications and of scientific and philosophic relations.

2. It must be easily acquired by every person of average elementary education, and especially by persons of European (and American) civilization.

3. It must not be one of the national languages.

Upon this program a widespread and effective propaganda has been developed. About 800 scientists of various countries, all university professors or members of scientific academies, and besides these about 200 societies of the most various kinds: scientific societies, chambers of commerce, touring clubs, etc., representing many thousand members, have joined the general movement. The purpose is, to ask after due time the Association des Academies, which represents the most important academies all over the world and is, therefore, the highest scientific corporation existing in our days, to take over this work of the international auxiliary language as its own, to appoint a working committee for the selection and introduction of such a language and to form a lasting and effective center for its culture and development. In case the association should decline this noble and important task, such a working committee will be selected by the above named international committee, formed by the representatives of

the different societies, etc., which have joined the movement.

Of the three conditions, the first two are self-explaining; only the third needs some discussion. The reason that any national language is considered as to be excluded is twofold. First, because the nationality whose language is chosen would obtain thereby an undue and unjust advantage over all other nations; it can not, therefore, be hoped that any such language will ever become truly international. The other reason is, that every natural or national language is sadly imperfect as compared with the possible ideal of a normal or regular language. The purpose of every language is to give a twofold coordination: one between ideas and sounds, and another between sounds and letters; by this means we can tell and write our ideas and can hear and read the ideas of our fellow men. To make such a coordination effective and consistent, it should be *univocal;* this means, that every idea should be coordinated only to one word, and *vice versa,* and that every sound should be coordinated only to one letter and *vice versa* again. Besides this, the coordination should be as simple as possible.

Of the natural languages none fulfils any of these conditions. Even if we abstract from the coordination between ideas and sounds (or written words) and consider only the coordination between letters and sounds, we never find it univocal. Italian and German are rather near to the ideal, because here the scripture is phonetic, but not yet fully so, while English is known as the worst possible language from this point of view. On the other side, Italian and German are very imperfect as to simplicity; they keep still the entirely useless differences of gender and express the same idea several times in the same sentence, for example, in coordinating to the plural in the subject superfluous plural forms in the adjective, the verb, the article and so on. English has made itself free from most of these superfluities, using only one article and almost no coordinations between the different members of a sentence. But there are still superfluous forms enough; the most frequent English phrase: 'how do you do?' giving a striking example.

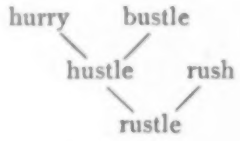
If there is no natural language, which fulfils the claim of simplicity and regularity, the question arises, if an artificial language may be constructed according to these conditions. Generally this question will be answered in the negative, but only on so-called theoretical reasons. To do away at once with probabilities, we will consider only facts. There exists an artificial language, called 'Esperanto' which is spoken and written by almost half a million people, which has about fifty periodicals and a stately collection of other books. A few months ago the Esperantists held their first international meeting at Boulogne, France, where 1,200 persons of twenty different nationalities were assembled, to try if it was possible to understand one another only on the basis of a language learned from books without oral teaching. This first experiment was in every way a grand success. There was not the slightest difficulty in understanding one another; even the English Esperantists pronounced the vowels quite clearly, in spite of their use or misuse in their own language. A play was performed, in which each player was from a different nationality and had learned his part at home, so that they met for the first time on the stage; the play went on without any difficulty for the players or the audience. Indeed, every possible application of a language was tried on this occasion, and all succeeded perfectly.

Space will not permit me to give a closer description of this language; every one, who desires to learn somewhat more about it, is kindly invited to apply to The Esperanto Club of Harvard University, Cambridge, Mass. By the same way information may be obtained as to the Delegation for the Adoption of an Auxiliary International Language, and its work.

Only one question can be answered at this place: will Esperanto be adopted by the delegation? It is impossible at present to say. Personally, I am inclined to believe that, indeed, Esperanto, either unchanged or after some slight modification, will be the auxiliary language of the future, but this is only a

personal judgment. Whatever language is chosen, it will be in any case the most important step mankind has taken since the invention of printing, for its future unification and for the saving of an enormous amount of hitherto wasted energy.

In the discussion which followed Professor Maurice Bloomfield, of the Johns Hopkins University, pointed out that the history of the second half of the nineteenth century in Europe was largely a history of re-nationalization. United Italy and Germany, Hungary and Bohemia are cases in point. Every minute and subjected people in southwestern Europe is engaged in furbishing up anew its national character. Each of them insists upon its own language as the chief sign of its individuality. Under these circumstances the hope that any one of the existing languages may become universal vanishes into thin air. The possibility of an artificial universal language comes to the fore, and the experiment deserves the most cordial support of all enlightened men. Nevertheless, Professor Bloomfield could not refrain from pointing out how much there is in language of organic growth, of mixed expression, which can not be produced artificially—chains of related and mixed words like 'hurry,' 'flurry,' 'scurry,' like



showing some of the mixed expressions which arise in speech through the subconscious creation of the individual and through the subconscious adoption of that creation by the masses. Simple idiomatic expressions like 'three years ago'—'Il y a trois ans,' 'vor drei Jahren'—illustrate how difficult it is to find common ground for even the most commonplace expression. Continuous issue of new words from a bureau of fabrication would exercise an incessant strain upon even the most willing disciples of an artificial language.

Professor Bloomfield was followed by Professor Mitchell Carroll, of the George Washington University, who spoke briefly in favor of Esperanto.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

The 405th regular meeting of the Biological Society was held on November 11, 1905, with Vice-President Hay in the chair and 36 persons present. Under brief notes and exhibition of specimens, Dr. B. W. Evermann said that while collecting yellow fever mosquitoes in Santo Domingo, Mr. August Busck had incidentally collected six specimens of fish, including four species, three of which proved to be new. Mr. W. H. Osgood exhibited an almost perfect molar tooth of a mastodon from Dawson, Yukon Territory, Canada. He stated that while mastodon remains had been found over a great part of the United States, the northernmost record up to the present time had been from near Lake Winnipeg, Manitoba. The tooth had been compared with specimens in the National Museum and found to agree with those of typical mastodons. Mr. W. L. Hahn showed a photograph of a red maple branch that had taken root in the ground and proceeded to grow. He also reported a specimen of the little brown bat, *Myotis lucifugus,* in the U. S. National Museum collection from Kamchatka, the previously most western record being from Kadiak Island, Alaska. The specimen was collected by Dr. W. H. Dall and proves to be typical *lucifugus.* Dr. C. W. Stiles reported that he had recently had sent to him for determination specimens of rat-tailed larvæ (*Aristilia*) alleged to have been passed by human patients. A case of pseudo-parasitism of this form is reported by Brera in 1809, and Dr. Stiles has recently examined specimens from two or three such cases in the United States. These specimens were also examined by Dr. L. O. Howard. Dr. Theodore Gill spoke of the wolf fishes of the genus *Anarrhichas,* of their distribution in the Atlantic and of their relationship. He finds that the species *donticalatis* and *latifrons* are generically distinct from the rest of the group. Mr. Walter H. Evans spoke of the introduction of minnows into the tero and rice ponds of Honolulu for the purpose of destroying the larvæ of the yellow fever mosquitoes. Dr. B. W. Evermann added that between 450 and 500 of these minnows, *Fundulus* and *Mollienesia,* transferred by Mr. Seale from Galveston,

Texas, to Honolulu were doing well and increasing rapidly. When liberated they immediately began devouring the mosquito larvæ. The *Mollienesia* are viviparous and soon after they were released many young were born. At the latest report the minnows numbered about 2,000.

The first regular paper of the evening was by Dr. E. L. Greene on 'Linnæus as an Evolutionist.' In the 'Philosophia Botanica' it was shown that Linnæus had clearly expressed his views that species were the same as when created in the beginning and as they were to remain for all time. In the later 'Species Plantarum' Dr. Greene was interested to find a query as to whether a certain species were not derived from another related species, and on reading through the 1,600 pages of this work he found numerous queries as to whether one species of plant had not been derived from some other species or acquired certain characters by adaptation to environment—soil or climatic conditions. Thus while in one place expressing an orthodox belief in special creation, his later querying is that of an advanced and pronounced evolutionist.

The second paper on the program was by Mr. Frederick V. Coville, on 'The International Botanical Congress at Vienna.' He said this was one of a series held every five years. It was attended by about 500 delegates, from the United States and most European countries, of which the German-Austrian group was greatly in the majority. The morning sessions of the meetings were devoted to presentation of botanical papers, the afternoon sessions to the adoption of a code of botanical nomenclature. The main questions brought up were: (1) The rejection of the Kew rule by adopting the principle of retaining the oldest specific name in whatever genus published; (2) an exception to the retention of the oldest specific name was made when it was identical with the name of the genus; (3) the American principle of rejecting homonyms was not adopted; (4) the principle of fixing upon a single species as the type of a genus was not accepted; (5) the principle of binomialism as a requisite for the publication of a genus was not adopted; (6) it was voted that after January 1, 1908, new names should not be valid unless accompanied by Latin diagnoses. Unfortunately, no preliminary meetings of the committee had been held for discussion and perfection of the code, and the proposals of the Americans to refer to committees points to which there was a large opposition was rejected.

Dr. Stiles discussed several points in connection with Mr. Coville's paper. He explained the organization of the international commission on zoological nomenclature and expressed the opinion that such organization was superior to the plan followed by the botanists, as the zoological plan not only provides for any necessary changes, but guards against any radical and unnecessary changes in nomenclatural customs. He thought that when zoological and botanical nomenclature were declared as independent of each other a very unfortunate mistake was committed, since such a rule was made without consideration of the numerous difficulties it would make for men working in the protozoa. However, now that the rule was generally adopted it was not feasible to attempt to rescind the rule. The zoological code provided, however, for certain cases which arise. He was of the opinion that the botanical provision that all new descriptions should be written in Latin was an unfortunate one and not capable of general practical application. He maintained that any man is able to write his own mother tongue more clearly and more concisely than he is able to express himself in a foreign language; further, probably not fifty per cent. of the men actively engaged in zoological and botanical work are able to read Latin with ease and certainly a smaller per cent. are able to write Latin. The rule in question as adopted by the botanists seemed to him not only a step backwards but further it played into a rather important local question in educational matters in certain countries and the rule would doubtless be cited by scientists in those countries in support of certain questions and legislation which were of purely local interest to those countries. It would further result in decreasing the activity of a number

of men in scientific work in case an attempt were made to enforce the rule. He believed, however, that the rule would be ignored by the majority of workers. Regarding the principle of tautonomy, he expressed himself not only as not opposed to it but as highly in favor of it, and he himself has decided to purposely introduce tautonimic names whenever the occasion presents itself. By use of tautonomy, the type species of a genus is shown in its name; without tautonomy, it is necessary to refer to the literature in order to recall the type species.

He did not believe it possible to enforce the rule concerning the list of excepted names. In fact, this rule did not seem to mean very much to him, as the congress had failed to determine the types for the names which were excepted and had apparently failed to provide for cases in which the genera in question might be defined in the future.

He recognized the delicacy of the situation which now faces the American botanists, but it seemed to him that the botanical code contained so many subjective elements that it was impossible for this code to expect to have a very long life. It has been the history of nomenclature since the time of Linnaeus that rules based on subjective ideas were short-lived. A rule in nomenclature must be objective if we expect it to be accepted generally and if we expect it to be permanent. Discussion followed by Dr. Gill.

The last paper on the program was by Dr. Hugh M. Smith on the 'Sargassum Fish,' a tropical species of the southern Atlantic occasionally brought by winds and currents to more northern shores. A large number of the fish were taken at Woods Holl and vicinity. The eggs from these prove to be entirely different from those described by Agassiz and long supposed to have belonged to the sargassum fish. Specimens of the fish, were shown and a beautiful painting of it by C. R. Knight, showing its wonderful protective coloration. The paper was discussed by Dr. Gill.

Vernon Bailey,
Recording Secretary, pro tem.

DISCUSSION AND CORRESPONDENCE.

ONTOGENETIC SPECIES AND OTHER SPECIES.

Referring to the admirable article in Science (November 24, 1905, p. 661) on 'The Evolution of Species through Climatic Conditions,' by Dr. J. A. Allen, I may once more gratefully recognize my own especial indebtedness to Dr. Allen's pioneer investigations of thirty years ago in this particular direction. These studies have been epoch-making in the history of ornithology.

It remains, however, to be determined whether these environmental forms—these species and subspecies produced by the direct influence of heat, cold, humidity and aridity—are 'ontogenetic species' (a term originating, so far as I know, with Professor V. L. Kellogg) or whether they have a real existence outside the lifetime of the individuals actually composing the group or species. We do not know which of the traits induced by direct section of the environment, if any, are actually hereditary and which are not. If we find that the dusky woodpeckers of Vancouver Island retain this shade when reared in Arizona, then humidity would be a real factor in the formation of species. If such birds, transferred in the egg to a new region should develop in the fashion of the local race of this region, and not like their own parents, then the duskiness is not a true specific or subspecific character. The real character of the species would be found in the tendency to develop dark plumage in humid surroundings and pale feathers under other conditions. In such case humidity would be merely a factor modifying individual development but not connected with the origin of species.

It may be that these questions have been already solved by experiment on birds, but if so, the experiments have escaped my attention. Eggs of the woodpeckers, chickadees and other birds showing dusky phases should be hatched in the arid plains. The red-shafted flicker of California should be bred in New England, and the permanence of the difference between large birds of northern range and their smaller southern homologues should be tested.

An 'ontogenetic species' its traits produced by the direct action of the environment, is the Loch Leven trout (*'Salmo levenensis'*), which I have lately discussed in these columns. Transferred to the brooks of England or to those of California, this supposed species loses its lake-bred characters and becomes the common brook trout.

Perhaps our ornithologists will some day test their species and subspecies by a test of the permanence of this class of characters. No doubt we should drop from the systematic lists all forms which may prove to be purely ontogenetic, all whose traits are not fixed in heredity.

In my recent article, as noticed by Dr. Allen, I have used the word 'barrier' a little too vaguely. For the purposes of this study, I should regard a broad plain as a barrier to a species which inhabits it, even though it were abundant, from one side to the other. A barrier in this sense is anything whatever which checks free interbreeding, even though it offers no actual check to the life or movement of the species. With quiescent animals, the individual moves but a short distance and the traits at one end of an unbroken series may be quite different from those of other individuals at the farther end, as Dr. Allen has very properly suggested. The term 'bionomic barrier,' used by Dr. A. E. Ortmann in a personal letter, seems to me a very apt one as covering the species-producing phases of isolation.

Certain papers of Rev. John T. Gulick on the evolution of species of land-snails and other animals deserve more attention than they have received. In one of these papers, 'Divergent Evolution through Cumulative Segregation' (Smithsonian Report for 1891, p. 273), Mr. Gulick corrects certain erroneous assumptions on the part of Dr. Moritz Wagner. Mr. Gulick says:

> Separate generation is a necessary condition for divergent evolution but not for the transformation of all the survivors of a species in one way.
>
> Separation does not necessarily imply any external barriers or even the occupation of separate districts.
>
> Diversity of natural selection is not necessary to diversity of evolution.
>
> Difference of external conditions is not necessary to diversity of evolution.
>
> Separation and variation—that is, variation not overwhelmed by crossing—is all that is necessary to secure divergence of type in the descendants of one stock, though external conditions remain the same and though the separation is other than geological. The separation I speak of is anything in the species or the environment that divides the species into two or more sections that do not freely intercross, whether the different sections remain in the original home or enter new and dissimilar environments.

All of this is in general accord with my own experience.

DAVID STARR JORDAN.

ORTHOGENETIC VARIATION?

IN a recent paper[1] I reviewed Gadow's hypothesis of 'Orthogenetic Variation,'[2] in the light of his own evidence, and in the light of such observations as could be added. In SCIENCE for November 7, 1905, Dr. Gadow publishes a reply under the title 'Orthogenetic Variation.' It would be superfluous merely to rediscuss the data previously published; in fact, had the matter gone no further than the original paper, elaborate criticism in the first instance might have been unnecessary, since scientific readers could judge the evidence for themselves. But unfortunately, as will be shown below, subsequent use has been made of the idea for presentation to the general public, not expressly as a tentative hypothesis —but *without qualification.*

In the first paragraph of his reply, Gadow says, 'I am anxious that it [orthogenetic variation] should not be misrepresented,' and, in the second paragraph, 'the paper by Mr. Robert E. Coker * * * calls for some remarks on my part by way of protest and correction.' I was glad that after careful reading of his paper, I found no reference to any statement

[1] Gadow's hypothesis of 'Orthogenetic Variation in the shells of Chelonia,' Johns Hopkins University Circular, No. 178, May, 1905.

[2] 'Zoological Results Based on Material from New Britain, New Guinea, Loyalty Islands and elsewhere, collected during the years 1895, 1896 and 1897, by Arthur Willey,' Part III., pp. 207–222, Pl. XXIV., XXV., Camb. Univ. Press, May, 1899.

of mine that had 'misrepresented' his hypothesis, and so needed 'correction.' In fact, I did not suppose I could have misrepresented it, because I had given it merely by quoting his own words at length. Two of the paragraphs quoted by me Gadow quotes in his recent paper, following them with the words "I think I had stated the case fairly. It left no doubt about the definition of at least one kind of orthogenetic variation" (p. 638). But we are indebted to his recent paper for the very concise statement that 'cases of orthogenetic variation are simply ontogenetic stages, passing reminiscences of earlier phylogenetic conditions.' The basis for his assumption that the abnormalities in number and arrangement of the horny shields of turtles are 'orthogentic variations' is a table of percentages of abnormalities, made from 76 specimens (47 new-born, and 29 of various sizes, from three inches to 'large'). This series of percentages of abnormalities, the percentage decreasing with age, is supposed to indicate that turtles 'amend their scutes' and 'grow out of these irregularities by the reduction or squeezing out of certain scutes.'

Gadow states (p. 639) that with the addition of my embryos to his 76 specimens, 'the percentage still decreases with age'; and gives the following revised table, including both sets of turtles:

	Per Cent.
Of 73[3] embryos or newborn, 53 are abnormal..	7[4]
Of 9 specimens from 3 to 8 inches, 3 are abnormal	33
Of 19 specimens from 8 to 24 inches, 5 are abnormal	22[5]
Of 9 specimens from 24 inches to 'large,' 2 are abnormal	24[6]
Of 7 large specimens, only 1 abnormal.....	12[7]

The table requires a comment. The last four groups are based, not on (9+19+9+7) 44 specimens, but on only 29, for 15 turtles were counted twice: the six 8-inch specimens in both second and third groups; the two 24-inch specimens in both third and fourth groups; and the seven 'large' specimens in both fourth and fifth groups. *Twenty-nine specimens divided into four groups, from which a series of per cents. is computed* to be compared with a per cent. based on a comparatively few newborn turtles—and this the *sole basis for an elaborate hypothesis,* given to the scientific world with the supporting (?) evidence and, subsequently, *given to the general public,* without the evidence, in a comprehensive monograph on 'Amphibia and Reptiles.' This I regard as the 'sole basis,' for though his comparison of the abnormalities with supposed phylogenetic stages is interesting and suggestive, and may support an interpretation of the abnormalities as *atavisms,* it does not in the least imply that *the individual recapitulates these stages,* and if the latter assumption has other basis than the table of percentages, what is it?

The writer is now pursuing anatomical and embryological studies, the results of which may have some bearing on the interpretation of the abnormalities in question, and these results will be given out in due time. But the question at present is not—Can Gadow's assumption be disproved? but—Have there been in hand facts to justify its promulgation? Being promulgated, should it be included *without qualification* in a comprehensive monograph intended for the general public, who will not refer to the original paper to find that it is merely a hypothetical assumption from a very small number of facts? The reference is to the 'Cambridge Natural History,' Vol. VIII., 'Amphibia and Reptiles,' by Hans Gadow (London, 1901), where the following unqualified statements occur (the italics are mine):

> It is absolutely certain that the number of transverse rows also was originally much greater than it is now. The mode of reduction of the numbers of the neural and costal shields has been studied in *Thalassochelys caretta* (cf. p. 388.) The accompanying illustration (Fig. 68) [This is a reproduction of text figure, from Willey's Zoological Results. R. E. C.] shows *some of the many stages actually observed* in the reduction of the

[3] Presumably typographical error. Gadow's 47 newborn plus my 28 embryos = 75.

[4] Presumably typographical error. 70 intended.

[5] Presumably typographical error. 26 intended.

[6] Presumably typographical error. 22 intended.

[7] Presumably typographical error. 14 intended.

shields. The chief point is that *certain shields are squeezed out, or suppressed by their enlarging neighbors.* The ultimate result is the formation of fewer but larger shields.[8]

Can these words be intended figuratively, the reference being to phylogenetic development, not to 'orthogenetic variation,' with all that that term, as defined by Gadow, implies? If so, the cross reference on a later page is certainly misleading: for in his discussion of the variations of *Thalassochelys caretta,* he says:

The interesting fact in connection with these variations is, moreover, that some of the shields are much smaller than the others, sometimes *mere vestiges in all stages of gradual suppression,* and that the *abnormalities are much more common in babies and small specimens than in adults. The importance of these 'orthogenetic variations' has been discussed on p. 326.*[9]

ROBERT E. COKER.

JOHNS HOPKINS UNIVERSITY,
November 28, 1905.

ON THE GRANTING OF THE M.D. DEGREE.

A SHORT time ago I received a letter from a member of a state board of medical examiners which touches upon a matter of present interest.

The letter, from which I shall quote, was in reply to one giving information respecting courses in this college designed for students who have the study of medicine in view.

After remarking that in his state the medical examiners had decided to give one year's credit to graduates of colleges, provided certain subjects in biology, chemistry and physics had been pursued in the college course, he proceeds as follows:

The fact is that many of the colleges teach these branches better than the average medical school. Any ordinary high school boy can enter the medical department of the university. Yet, they are not willing to give a year's credit to men who take four years beyond their entrance requirement. The confederation of state medical boards is divided on the question. So long as the average medical school admits high school graduates, I shall stand for giving one year's time to men who take a college course. Or, in other words, seven years for the combined medical and college course. Not six years as proposed by Michigan, provided men take both courses at Ann Arbor. The seven years seem to me to be only fair play as an encouragement to the higher education.

What I wish to write you about in particular, is this: The present regulation is not to give the college men any time credit. The plan originates with medical schools in universities where they have also an arts department. They do allow the medical and college course to be completed in six years instead of eight, but it requires men to go to their college department. Now there are several medical schools requiring a straight B.S. or A.B. degree for entrance, such as Johns Hopkins, Harvard, and Rush in 1907. If men going from colleges * * * will all go to schools requiring the A.B. or B.S. entrance requirement, it will do more to help us to bring the medical schools into line than anything I know of at present. It seems to me the professors in these colleges should bring every pressure to bear on their prospective medical students to get them to go to the medical schools only that require degrees for entrance.

Upon the question of requiring either the B.A. or the B.S. degree as a preliminary to a medical course it is not my purpose to speak further than to say that I do not think the time has come in this country to make such requirement, unless upon the completion of such course the degree M.D. is to be given.

President Hadley has this to say on the general subject of requirements for admission to the professional schools of Yale:

However convenient it might be to insist on the possession of a bachelor's degree by all pupils in the schools of law or medicine, I feel that it would be a violation of our duty to these professions to hedge ourselves about by any such artificial limitations. We should make the standard of admission to our law and medical schools higher than it is at present; but we should base it upon qualifications for professional study which we could test by an examination, rather than upon previous residence at an institution entitled to give a bachelor's degree. If a man is really fit to study law or medicine we should encourage him to study law or medicine with us, without making arbitrary restrictions.

No one will be likely to question the wisdom of President Hadley's remarks, provided the

[8] *Loc. cit.*, p. 326.
[9] *Loc. cit.*, p. 388.

degree to be given at the end of the course in law or medicine is the bachelor's degree.

If, however, the doctor's degree is to be given at the end of the course in law or medicine, then an entirely reasonable prerequisite, a prerequisite which is justified by the generally approved custom in the case of the Ph.D. degree, would be to require the possession of a bachelor's degree.

Certain of the smaller colleges that still occasionally give the degree of Ph.D., without requiring residence study, have had the enormity of their educational transgressions pointed out to them by those interested in university work.

Of course, the practise of giving the Ph.D. degree as an honorary degree is not to be defended and would receive no encouragement if the conferring of the degree were referred to the teaching force of our colleges, instead of being kept as a prerogative of boards of trustees.

But there are other offenders against good taste in the matter of conferring degrees beside the small colleges; and one is tempted to remark that the practise of certain universities of offering as inducements to students to come to them, the opportunity of shortening their combined undergraduate and professional course, smacks somewhat of the bargain counter.

Any institution which makes such offer of course pays the price for any advantage which it may gain in increased attendance, in the lower place which it thereby takes in the estimation of a critical public as compared with institutions which do not resort to such devices to lure students to their halls.

The one point, however, which I wish to emphasize in the present communication is this: The doctor's degree should be a second degree and should not be given, unless as an honorary title, to any one who has not earned a bachelor's degree.

This proposition will not be dissented from by any university when the degree of Ph.D. is in question. Why should there be any hesitation then in requiring a bachelor's degree as a prerequisite to the M.D. degree?

At many universities two boys, presenting themselves with the same preparation, may enter on the same date; the one registering in the undergraduate department, the other in the medical school. At the end of four years the one receives the bachelor's degree, the other the doctor's degree. Such anomalous conditions can not be justified on educational grounds.

It follows that the doctor's degree from a school of medicine which does not make the possession of a bachelor's degree an entrance requirement is, judged by educational standards, on a par with the bachelor's degree. Graduates of such schools, therefore, should be given the degree of bachelor of medicine instead of doctor of medicine.

I am not arguing that a bachelor's degree in arts or science should be required of every one who proposes to practise medicine. I am not discussing the question of the desirability of having the practitioner a broadly educated man. Neither do I propose to discuss the claims made in behalf of certain medical schools that they turn out better trained practitioners without requiring a bachelor's degree for entrance than is done by schools which require a bachelor's degree for entrance. Claims of this sort are easily made. The discriminating public will judge the institution by what the graduates prove to be rather than by what they are said to be.

While I do not think we are ready in this country to say that no one should be permitted to practise medicine until he has gained the degree of M.D., we may and should insist that he be a graduate of a reputable medical school, and that he pass successfully the examination set by the state board of medical examiners

It is, perhaps, not too late to insist that the degree M.D. be put on a par with the degree Ph.D., instead of ranking, as it does now, except as it comes from a very small number of medical schools, with the bachelor's degree.

Edwin Linton.
Washington and Jefferson College,
October 13, 1905.

THE PROPOSED BIOLOGICAL STATION IN GREENLAND.

To THE EDITOR OF SCIENCE: It was my pleasure some time ago to communicate to the readers of SCIENCE that the money for the erection of a biological station in Greenland is donated by a citizen of Denmark. I am now able to add further that it is donated by Herrn Justitsraad P. Holck, of Copenhagen. He has donated 35,000 Kroner (about $10,000) for that purpose, which is the estimated cost, according to Magister M. P. Porsild's plan. It now remains for the Danish government only to appropriate money enough for its running expenses, which, according to Mr. Porsild's plan, will amount to 11,000 Kroner (about $3,000) a year.

To show the great interest which scientists have taken in Magister Porsild's plan I could mention the numerous articles in Danish, Swedish, German and American scientific journals, and the recommendations from leading biologists and polar explorers, as Professor Fridthof Nansen, Sir Clements Markham, Professors Vanhöffen, von Drygalski, Aug. Warming, K. V. Steenstrup and F. Jungersen, Drs. Gunnar Anderson and Georg Brandes, and many others. In the United States it has been highly recommended by Professors E. B. Wilson, C. O. Whitman, J. Loeb, T. H. Morgan and Mr. Pehr Olsson-Seffer, Ph.D.

Sir Clements R. Markham, president of the Royal Geographical Society in London, says: "Your proposal to establish a biological observatory is, in my opinion, deserving of support. I believe that the science of geography would be advanced by it, and would derive important results. * * * I wish you all possible success in your efforts to secure that result ever."

Professor Dr. Vanhöffen, in Petermann's *Geogr. Mittheilungen,* says: "A kind of biological station was established in Greenland in the time of Holbölls and Rinks, the former remained there eighteen years, the latter twenty years. They were the central figures in the exploration and investigation of Greenland. Especially Mr. Rinks knew the people of Greenland well and we have as a result his classical work, 'Grönland, geografisch und statistisch beschrieben.'" After a longer description of Mr. Porsild's plan, he states: "It is a very extensive plan that the leader of the future arctic biological station has; but, without doubt, great results are to be expected from such a station."

The north polar explorer Professor Fridthoff Nansen says: "A biological station in Greenland will be of immense value for scientific research. Especially the investigation of its fauna and flora can not help to give results of utmost importance to biological science."

As soon as the Danish government has decided to appropriate money for the support of the station I shall publish an article and describe the proposed function and the opportunities offered to investigators in the first arctic biological station.

At present we can only express our sincerest thanks, high respect and gratitude to men of science in foreign countries who have shown their interest in this Danish scientific undertaking, which we hope will add greatly to the universal advance of biological, geological and geographical science.

M. E. HENRIKSEN.

COLUMBIA UNIVERSITY,
December 1, 1905.

SPECIAL ARTICLES.

THE ASSUMED PURITY OF THE GERM CELLS IN MENDELIAN RESULTS.

CUÉNOT has recently published[1] in the fourth note of his important series of experiments with mice, certain results with yellow mice that are of fundamental importance as bearing on the question of the assumed Mendelian purity of the germ-cells. His results and his interpretation of them are briefly as follows:

Wishing to obtain 'pure' yellow mice, he crossed yellow mice known to be heterozygotes (*i. e.*, containing recessive gray or black in this case). According to the Mendelian formula he should have obtained the following results:

$$CYCG \times CYCG = 1\,CYCY + 2\,CYCG + 1\,CGCG.$$
(Yellow, Yellow, Gray)

[1] 'Les Race Pures et Leur Combinaisons chez les Souris,' *Arch. de Zool. Exp. et Gen.*, 1905, Vol. III., Notes et Revue, page cxxiii.

The letter C in this formula stands for any color (in contrast to white, A = albinism); G indicates gray, and Y yellow-bearing gametes. The formula shows that 25 per cent. should be pure yellow, 50 per cent. yellow-grays (*i. e.*, dominant recessives), and 25 pure grays. To his surprise he got no pure yellow mice, CY, but only impure yellows (*i. e.*, yellow dominants, $CYCG$). Quantitatively there were 72.7 per cent. yellows, $CYCG$, and 27.3 per cent. 'pure' grays.

Why were no pure yellows obtained? In other combinations yellow seemed to follow the same rule as the other colors, gray, black or brown, but neither the pure yellow CY, nor the albino with yellow alone recessive, AY, nor the yellow heterozygote $CYAY$ could be obtained.

Cuénot explains these curious results by means of the assumption that the gametes (eggs or sperm) having the formula CY or AY never unite to give the forms $CYCY$, $AYAY$, $CYAY$. In other words, yellow-bearing spermatozoa never unite with eggs bearing the yellow color alone. If this conclusion could be established with certainty it would have a fundamental bearing on all questions connected with Mendelian phenomena. It is, therefore, of the utmost importance to know whether Cuénot's interpretation is the only one that can be given to his results.

It seems to me, in the first place, highly improbable that such a trivial difference as the color of the hair could absolutely prevent the conjugation of the gametes carrying these colors, especially when we recall that the same gametes must carry thousands of other unit-characters that are identical.

It seems also more *probable* that the results mean, that the color, yellow, was never disassociated from one of the other colors, gray, or black, or brown; and that the results are due to this and not to the disjunction of yellow and its failure to combine with yellow again. On this basis, moreover, all the facts can be explained according to the Mendelian formula. Incidentally the whole question of what is meant by the '*purity*' of the germ cells is here raised. Let me now try to show how these claims can be justified.

Cuénot obtained his yellow mice by crossing an albino of unknown ancestry with colored mice. His yellow mice must, therefore, have contained gray (or black). Now the theory of 'pure' germ-cells *assumes*, in order to explain the Mendelian ratio, that the germ-cells of $CYCG$ separate into two sorts, CY and CG. I *assume*, on the contrary, that the mixed characters do not separate again, but alternately dominate and remain latent giving $CY(CG)$ and $(CY)CG$. Two mice of this kind paired will give:

$$\begin{array}{cccc} & CY(CG) & (CY)CG & \\ & CY(CG) & (CY)CG & \\ 1\,CY(CG) & +\,2\,CY(CG) & (CY)CG & +\,1\,(CY)CG. \\ \text{Yellow} & & \text{Yellow} & \text{Gray} \end{array}$$

Thus there will be three yellows to one gray. There are, moreover, two classes of yellows, but while the first group $CY(CG)$ will breed true, the other group $CY(CG)(CY)CG$ will split in each successive generation according to the Mendelian formula. There is an implication in this point of view of importance for the conception of germ-purity. The gray in the latent condition in $CY(CG)$ is different from the gray in the recessive condition in $CY(CG)(CY)CG$, for the former remains in the latent condition to CY in inbreeding, while the latter, the free CG, becomes dominant in half of the germ cells. Thus the germ cells of $CY(CG)$ are all $CY(CG)$, but half the germ cells of $CY(CG)(CY)CG$ are yellow, $CY(CG)$, and half gray, $(CY)CG$. The same rule applies to the 'pure' grays $(CY)CG$ in which the latent (CY) always remains latent in inbreeding. Crossing with other races may, however, bring the yellow from its latent position to dominance or to recessiveness again.

Cuénot's yellow mice were, on my view, either $CY(CG)$ or $CY(CG)(CY)CG$. The former if inbred should give usually yellow mice, but if crossed with mice of other colors, or united with $CY(CG)(CY)CG$, would give some grays. Cuénot's somewhat ambiguous statement on the top of page cxxix may accord with this view, although it may have a different meaning. He obtained no pure yellows, $CYCY$, because, on my view, his yellow mice were contaminated at the start.

The result is exactly the same as when a so-called 'pure' Mendelian gray mouse is obtained through a cross. The 'pure' dominant is contaminated in the same sense as the 'pure' recessive and in the same way. For example, a gray mouse crossed with a black will give a mouse $CGCB$ whose germ cells will be, on my view, $CG(CB)$ and $(CG)CB$. Such mice inbred will give

$$1\,CG(CB) + 2\,CG(CB)\ (CG)CB + 1\ (CG)CB.$$

The so-called 'pure' dominant, $CG(CB)$, is really only a dominant gray with black in the latent condition. This form is strictly comparable to the yellow mice that Cuénot tested for purity, and could not be expected to give by inbreeding a form with perfectly pure germ cells, $CGCG$. To obtain such a pure gray mouse, $CGCG$, a wild form, never crossed with black, must be sought; for, once crossed always contaminated.

The preceding results given for these special cases are strictly in accordance with what is now becoming recognized in regard to the usual condition of albino mice. It has been shown that these carry gray, or black, or some other color *latent*. Thus a white mouse is $A(G)$ or $A(B)$ or $A(Y)$. Suppose one of these white mice, with the formula $A(G)$, is crossed with a pure gray mouse, with the formula CG:

$$A(G) \times CG = A(G)CG.$$

The offspring are all gray, because free gray dominates free white. Suppose now we inbreed these $A(G)CG$ mice. Their germ cells will be, on my point of view, of two sorts, viz., $A(G)(CG)$ and $(A(G))CG$. The resulting offspring give

$$\underset{\text{White}}{1\,A(G)\,(CG)} + \underset{\text{Gray}}{2\,A(G)\,(CG)\,(A(G))CG} + \underset{\text{Gray}}{1\ (A(G))CG}.$$

Since albino is white, while C stands for the color with which it is associated, we may simplify the form of the equation by omitting the C's and putting the G's together, thus:

$$1\,A(G) + 2\,A(G)\,(A(G))G + 1(A(G))G.$$

Thus our formula gives the Mendelian results, but it also brings to light the different relation of latent (G) and free G. If individuals of the middle group above be inbred, they again give the Mendelian ratio 1:2:1; because the free A and the free G alternately dominate and become latent, so that the germ cells are $A(G)$ and $(A)G$.

It is needless to go over the well-known Mendelian combinations of other sorts, back crossing, etc., since they also work out according to our formula.

The important point is not that there is offered here a new set of Mendelian formulæ, but a new conception regarding dominance and recessiveness, which I believe to be better in accord with the conditions found to exist in extracted recessives. Furthermore, this idea brings into question the assumption of the so-called purity of the germ cells, by means of which modern writers are explaining the Mendelian results. Purity only means dominance over latency. Dominance over recessiveness follows a different rule, viz., the rule of alternation or of contrasted gametes.

T. H. MORGAN.

RECENT CHANGE OF LEVEL IN ALASKA.[1]

ABOUT the middle of September, 1899, the coast of Alaska, in the St. Elias-Fairweather region, was visited by a series of vigorous earthquake shocks, one effect of which was to greatly modify the front of the Muir glacier, and to fill Muir Inlet with ice dislodged from the glacier. The shocks were also felt with marked intensity at Yakutat, and a company of prospectors, camped near the Hubbard glacier in Disenchantment Bay, report a series of severe shocks and accompanying water waves.

[1] The observations outlined in this paper were made in the summer of 1905 in connection with a general geological survey of the Yakutat Bay region by a United States Geological Survey party under the direction of the senior author. A grant of money obtained through the assistance of the American Geographical Society made it possible to add the junior author to the party as special assistant in physiographic and glacial geology. Acknowledgments are due to B. S. Butler, the other member of the party, for assistance in this work. Published by permission of the Director of the U. S. Geological Survey.

During our work along the shores of Yakutat Bay, and its extension called Disenchantment Bay and Russell Fiord, abundant evidence was found to prove that the shores of the great fiord suffered a profound disturbance during this earthquake. The evidences of change of level are: (1) physiographic—elevated rock benches, sea caves, chasms, alluvial fans and beaches, submarine till uplifted to form shore lines, and the appearance of new reefs and islands; (2) biological—various marine forms, such as *Balanus, Mytilus* and a calcareous alga clinging to uplifted rock on which land plants now thrive, but in no case plants more than five years in age; and (3) human—the narrative of the Yakutat natives. Depression is shown by the sea encroaching on and killing trees; and extensive water waves are proved by the destruction of forests along the shores of Yakutat Bay high above the present reach of the waves.

The fiord, which is about sixty miles in length, has the form of a bent arm, starting from the Pacific as the broad Yakutat Bay. This bay is bordered by a low foreland of glacial debris on the southeast and by Malaspina glacier, with its fringe of glacial debris, on the west.

Back of the lowland fringe rises a range of mountains, really foothills of the St. Elias chain, reaching elevations of from 4,000 to 6,000 feet. The seaward face of this range rises abruptly out of the low foreland with a steep and remarkably straight front. Into this range Yakutat Bay penetrates, rapidly narrowing to the form of a fiord whose lower end is named Disenchantment Bay. At the head of this bay the fiord, thence called Russell Fiord, abruptly turns back toward the ocean and extends out beyond the steep mountain front, ending in a bay-like expansion in the fringing lowland. Three large tidal glaciers —Hubbard, Turner and Nunatak—fed among the lofty mountains of the St. Elias chain, enter the fiord.

The mountainous shores of the fiord have been differentially deformed, some parts showing no change in level, while one shore, that south of Turner glacier, has suffered elevation of from 33 to 47 feet. Accompanying this change there has evidently been faulting along some of the arms of the fiord. That this faulting has been complex is indicated by the presence, in a number of places on the land, of numerous new faults close together and parallel to each other and, in general, to the strike of the St. Elias chain. The best of these were observed on the nunatak at Nunatak glacier, where the maximum throw of single faults reached three feet, though the aggregate throw of all the faults in a given region amounted to several times this figure. The region has apparently been warped and broken into a series of blocks which are tilted in a complex relation, and which themselves have suffered extensive minor faulting. In one place, along the lower part of Russell Fiord, the faulting apparently follows a line of earlier movement.

The uplift is confined to that part of the region which lies along and inside of the steep, straight mountain front, while on the low fringe of foreland, and in the Malaspina glacier region, there is, for the most part, no evidence of any change of level excepting at the very mountain base. Locally, however, and especially near the mountain, there is depression along the shores of the lowland, reaching a maximum of from three to five feet. Professor Russell long ago assigned to this mountain front a fault origin on the basis of the form, and our observations confirm this interpretation. Moreover, there is clear evidence of a still earlier, though very recent, movement along this major fault line.

Our observations, which later will be published in full, indicate that the earthquake was the result of a great upward movement along the front of the St. Elias chain, with minor differential movements of large and small kind, together complexly fracturing the crust along and near the shores of the fiord. Along the extreme front of the mountains the uplift was from six to nine feet; but further back it locally reached a much greater figure, in one place, as previously stated, causing an uplift of the coast of 47 feet.

Ralph S. Tarr,
Lawrence Martin.

Cornell University.

BOTANICAL NOTES.

THE AMERICAN BREEDERS' ASSOCIATION.

A COUPLE of years ago, a number of botanists and zoologists joined interests with a number of practical growers of plants and animals and effected an organization of a new society under the name of 'The American Breeders' Association.' The purpose, as set forth in the constitution, adopted at St. Louis, December 29, 1903, is 'to study the laws of breeding and to promote the improvement of plants and animals by the development of expert methods of breeding.' With this object in view, about seven hundred members have been enrolled. These are, naturally, quite largely drawn from the ranks of practical breeders, but there are many scientific men also, and it is notable that the officers of the association have been very largely chosen from the professors in universities and colleges, and the scientific experts in the service of the United States Department of Agriculture.

Two meetings have been held, the first in St. Louis, December, 1903, and the second in Champaign, Illinois, February, 1905. In the first, seventeen papers were read, fully half of which possess more or less interest to the botanist. In the second meeting, twenty-eight papers were presented, with a still larger preponderance of papers relating to plant breeding. Among the papers of especial interest to botanists may be noted one by Professor DeVries on 'Investigations into the Heredity of Sporting Varieties,' another by Doctor Webber on 'Cotton Breeding,' one by W. A. Orton on 'Plant Breeding as a Factor in Controlling Plant Diseases,' one by Professor Hansen on 'Breeding Mildew-resistant Sand Cherries and Roses' and one by Dr. George T. Moore on 'Breeding Bacteria.' Other titles equally suggestive to the botanist might be cited, but these will show what the approaching meeting in Lincoln, January 17, 18 and 19, is likely to include. The program for this meeting is now being made out. It is understood that, in addition to general sessions of the association, there will be joint sessions with the State Horticultural Society, Corn Growers' Association, Live Stock and Dairymen's associations. The secretary is Professor W. M. Hays (assistant secretary of agriculture), Washington, D. C.

METHODS IN PLANT HISTOLOGY.

A LITTLE more than four years ago, the first edition of Dr. Chamberlain's 'Methods in Plant Histology' was noticed in these columns (SCIENCE, August 16, 1901). Now a second edition has appeared, much improved and considerably enlarged. In it so much new matter has been incorporated that the book is in fact a new one and must wholly replace the first edition. The extent of the enlargement may be estimated from the fact that, while the first edition contained 159 pages and 74 figures, the second contains 262 pages and 88 figures. Many new methods of work are described in this edition which were but briefly referred to, or were wholly omitted from the first. In the systematic part of the book many more suggestions as to collecting and growing material are given, thus greatly increasing its practical value. This edition must prove to be even more useful to botanical workers in high school, college and university laboratories than its predecessor.

FERNS OF THE PHILIPPINE ISLANDS.

DR. E. B. COPELAND has recently made an important addition to our knowledge of the flora of the Philippines by the publication of a descriptive list of the orders of ferns (*Polypodiaceae*) occurring in the islands. It appears as Bulletin 28 of the government laboratories at Manila, bearing date of July, 1905, and covers 139 octavo pages. The general nature of the fern flora as far as represented in this booklet may be understood from the following analysis. There are eight families represented, as follows: *Woodsieae* (with 1 genus, and 1 species); *Aspidieae* (9 genera, 117 species); *Davallieae* (12 genera, 72 species); *Asplenieae* (12 genera, 91 species); *Pterideae* (11 genera, 44 species); *Vittarieae* (3 genera, 14 species); *Polypodieae* (10 genera, 99 species); *Achrostichieae* (4 genera, 7 species). The largest genera are *Nephrodium* (with 60 species), *Lindsaya* (with 22 species), *Asplenium* (with 36 species), and

Polypodium (with 73 species). Two species of the interesting and curious 'staghorn ferns' (*Platycerium grande* and *P. biforme*) occur on the islands.

SOME NOTEWORTHY BULLETINS.

Professor B. M. Duggar's paper on 'The Principles of Mushroom Growing and Mushroom Spawn Making' has been issued as Bulletin 85 of the Bureau of Plant Industry, of the United States Department of Agriculture. It covers sixty pages and includes seven half-tone plates. It will be useful to botanists and especially so to the growers of mushrooms.—Bulletin 84 of the Bureau of Plant Industry, entitled 'The Seeds of the Bluegrasses,' contains a paper by Edgar Brown on the germination, growing, handling and adulteration of bluegrass seeds, and another by F. H. Hillman, consisting of descriptions of the seeds of the commercial bluegrasses, and their impurities. Illustrations in the text add much to the value of the bulletin.—O. F. Cook and W. T. Swingle discuss the 'Evolution of Cellular Structures' in Bulletin 81 of the Bureau of Plant Industry. It is a discussion of the mode of evolution, and lays particular emphasis upon *symbasis,* that is, diversity of descent with normal interbreeding. They say 'species are sexual phenomena; they have come where they are only through symbasis; that is, as groups of interbreeding individuals, traveling together along the evolutionary pathway.'—From the same bureau, we have in Bulletin 90, part II., a short paper by G. G. Hedgcock on 'The Crown-Gall and Hairy-root Diseases of the Apple Tree,' in which the author separates the two, establishes the non-contagious nature of the first, says that there is no proof that the second is contagious, and shows by experiments that the first affects the growth of the tree little if any. The paper is in the nature of a report of progress and is very suggestive to plant pathologists and practical orchardists.—Bulletin 64 of the Forest Service, by Raphael Zon, deals with the characteristics, growth, distribution and uses of the loblolly pine (*Pinus taeda*) in eastern Texas. Especial attention is given to its use in the production of railway ties.—In Bulletin 28 of the Bureau of Soils, B. E. Livingston, J. C. Britton and F. R. Reid give the results of their 'Studies on the Properties of an Unproductive Soil,' and reach the rather startling conclusion that the particular soil studied (at Takoma Park, Md.) 'contains a water-soluble, non-volatile substance or substances, probably organic in nature, which are toxic to wheat plants, causing a stunting of their growth.'—From the United States National Herbarium we have (Vol. VIII., pt. 4) the fourth of a series of 'Studies of Mexican and Central American Plants' by Dr. J. N. Rose, the result of a fourth journey to Mexico, made by the author. It contains many descriptions of new species, and critical notes upon old ones. It is illustrated by ten plates and six text figures.

Charles E. Bessey.
The University of Nebraska.

CURRENT NOTES ON METEOROLOGY.

KITE-FLYING OVER THE TROPICAL OCEANS.

Reference has frequently been made in Science during the past two or three years to the project for exploring the atmosphere over the tropical oceans by means of kites, suggested by Mr. A. Lawrence Rotch, of Blue Hill Observatory, in 1901. It is pleasant to be able to chronicle, in these notes, the successful ending of a preliminary expedition undertaken during the past summer under the joint auspices of Mr. Rotch and of Mons. L. Teisserenc de Bort. Preliminary reports have appeared in the *Comptes Rendus,* October 9, 1905, 'Sur les Preuves de l'Existence du Contre-Alizé,' by Rotch and de Bort, and in *Nature,* November 16, 1905, 'The Exploration of the Atmosphere over the Tropical Oceans,' by the same authors. One of the chief objects of the expedition was to study the anti-trade winds from the southwest, which, according to a report by Dr. Hergesell, based on his observations in 1904, do not exist. The work last summer was done on board the *Otaria,* a steamer equipped with an electric kite-reel already used by de Bort for kite-flying at sea. Messrs. Clayton, of Blue Hill, and Maurice, assistant at the observatory at Trappes, carried out the exploration.

Paper pilot balloons, to show wind directions and not carrying instruments, were sent up and their height was obtained by simultaneous angular measurements at the end of a base-line on shore. Soundings of this kind were made at Madeira, Teneriffe and the Cape Verde Islands in particular, and also over the open ocean. Observations were also made on the peaks of Teneriffe and Togo, and included the drift of clouds above these peaks. Tabulations of these observations, and a chart showing the tracks of three balloons, make it evident that the winds blowing equatorward vary in direction between northeast and northwest. The latter are usually above the northeast stratum, the thickness of this layer of the trades near Teneriffe being about 3,000 or 5,000 meters. Above this there are southeast, south and southwest currents—the anti-trades—of great thickness, probably, but of small density. The east wind near the thermal equator extends very high, as had previously been inferred from the carriage of volcanic dust and the movements of lofty clouds. The southeast wind was observed by a balloon at the Cape Verde Islands up as far as 6.8 miles.

The results obtained during the past summer, therefore, confirm the accepted theory of trades and anti-trades, and are not in accord with the view advanced by Dr. Hergesell. North of Madeira and towards the Azores the upper winds, as already shown by cloud observations, are prevailingly from west and northwest, this region being usually north of the oceanic high pressure area and outside the trade wind zone. The anti-trades, with their southerly component, are generally southwest in the latitude of the Canary Islands and southeast near the Cape Verde Islands, corresponding to the effects of the earth's rotation. Upper cloud observations at Havana and in the Antilles further prove the existence of these winds.

AN INSTRUMENT FOR DETERMINING TRUE WIND DIRECTIONS AND VELOCITIES AT SEA.

IN the 'Report of the Imperial Academy of Sciences of St. Petersburg' for August, 1905, Mr. Rotch describes an instrument of his own invention which is designed to give the true wind direction and velocity on a moving vessel at sea, an observation which has always been difficult and troublesome. The theory of this instrument is based on the triangle of forces. It consists of two movable discs, graduated into 360° each, and three rulers, hinged so that they may be adjusted to different angles. One of these rulers indicates the direction in which the vessel moves; a second shows the apparent, and the third the true wind direction. To set the instrument the direction of movement and the speed of the vessel; the apparent wind direction, shown by the smoke from the funnel, and the true wind direction shown by the waves, are needed. When the rulers are set to accord with these conditions, the true wind direction, in degrees, and the true wind velocity, in miles an hour, are shown by means of the discs above referred to.

Casella, of London, is the maker of the instrument, the price of which is approximately £3.

BRITISH RAINFALL, 1904.

THE forty-fourth annual volume on 'British Rainfall,' compiled by Dr. H. R. Mill, includes the records of 3,982 rain gauges. In his preface the compiler regrets that he can not visit more of the stations, and notes that the chief difficulty is the inadequate train service in the rural districts. The effect of the automobile's advent is noted in the statement that 'a few years ago the bicycle supplied a quick and easy means of overcoming the difficulty, but now, except for the fact that it is not prohibited by law, cycling on the high roads of England scarcely differs in point of danger from walking on the railways.' The volume contains an appreciative mention of the valuable work done at Ben Nevis Observatory since 1883, and now unfortunately given up, and the frontispiece shows the summit of the mountain in winter. There are other papers on subjects of more local British interest, but we call attention with special satisfaction to the charts showing the tracks of cyclones which produced exceptionally heavy rainfalls during the year 1904, the same charts showing also the distribution of the

rainfall. This is an emphasis on the cyclonic unit which we have long hoped to see.

R. DEC.

SOME STATE CENSUS FIGURES

THE state censuses for 1905 a[illegible] some instructive returns. Iowa, for [illegible]nce, shows a loss of 15,000 persons since 1900. The cities of 5,000 population and over gained 77,000 people in all; the towns under 5,000 and the rural districts together report a loss of 92,000. In Minnesota, where the gain during the decade, 1890 to 1899, inclusive, was 33.7 per cent., there has apparently been a slowing up. The decennial rate was 3.37 per cent. a year; but for the past five years, 1900 to 1905, there was a gain of only 13 per cent., or 2.6 per cent. a year. As the basis broadens the rate of accretion neces[illegible]arily becomes slower, while in Iowa the rate indicates even retrogression. The indications are that, either from urban migration or from other causes, or from all combined, the farming population even in the most prosperous portions of the west has practically ceased to grow.

One reason for this, if the view of arrested growth be accepted, is to be found in the rapidly rising price of farming land. For the past several years or more the trend of prices of land has gone upward with the prices of farm produce. Iowa, being a dairying and stock-growing state, has come to put such values upon her farm lands as to dislodge the old style of farming for a family home, in favor of the capitalistic farmer—the farmer who puts surplus income back into land, into better methods of cultivation, better stock and better facilities. The old-style farmer moves off to Canada for frontier lands, or to the southwest or northwest, where land is cheaper, after having reaped the reward of waiting, in the form of the unearned increment.

Kansas took her fifth decennial census on March 1, 1905, and found the insignificant increase of 8,658 persons in one year, the total population being 1,543,868. This gives an average of 14,703 people for each of the 105 counties. Of these counties 58 report an increase, and 47 a decrease, compared with March 1, 1904. The highest increase is 2,987 persons out of a total of 48,058, or 6.6 per cent. gain in one year. The largest decline is one of 2,087 persons, leaving a population of 24,907, or 9.1 per cent. less than a year earlier. These are marked changes to occur in so small a population in the course of twelve months from ordinary causes in times of prosperity in city and country alike.

Turning from country to city, we see that in Kansas towns the same shifting is going on. One might think that towns have been the gainers of country losses; but this is not always the case. The changes are due to a wider range of influences than urban attraction. Of 119 cities of a thousand inhabitants and over, 61 gained in the last year and 58 lost in numbers. Only four gained over one thousand each, and five of the cities lost each a thousand or over; but none so much as two thousand. While these are small numbers, they indicate the presence of some active influences which are responsible for a great deal of readjustment. Kansas is eminently the commonwealth of comparatively small towns. How emphatically this is the case is apparent from the following table of towns of 1,000 people and over, which may or may not suggest some explanation of the gain and loss account within its borders:

No. of Towns.	Range of Population.
4	have each from 20,924 to 67,613 inhabitants.
8	have each from 11,190 to 18,257 inhabitants.
12	have each from 5,188 to 9,899 inhabitants.
40	have each from 2,013 to 4,427 inhabitants.
55	have each from 1,009 to 1,998 inhabitants.

Any attempt to trace these evidences of arrest in increase, or of decrease, to unfavorable agricultural conditions must fail; because the same tendency appears in manufacturing states. For instance, Massachusetts, which is more than half cities or towns of over 5,000, has a disappointing return in its census for 1905. Taking into account the increase in the previous decade, a growth in population of 375,000 was expected and predicted. The actual increase is 193,612, barely half the expected gain.

The rate of increase, 6.9 per cent., is half that for twenty years back. It was 7.9 per cent., 1875 to 1880; and 8.9 per cent., 1880 to 1885; but this is the only time it has ever been as low; and of the earlier decade, the first five years, 1875 to 1880, were years of great financial depression.

The past five years have been years of overflowing prosperity. Yet the increase in population in Massachusetts drops to one half the earlier rate. The addition to population in the latest five years is no larger than it was thirty years ago, when the inhabitants of the states numbered only half as many as now.

"This same arrest of population," says the Philadelphia *Press*, "is in progress all over the country. No state is likely to show in this decade the increase of the past. Our national increase, which has been jogging along at about 25 per cent., in ten years, is about to make a drop to 12 or 15 per cent. in ten years, a little above the average of thriving European countries like Germany and England."

JOHN FRANKLIN CROWELL.

WASHINGTON, D. C.

THE MUSEUMS ASSOCIATION OF AMERICA.

AN informal meeting of a few of the administrative heads of some of the greater museums of America was held at the United States National Museum in Washington on December 21. There were present Dr. Richard Rathbun, the director of the National Museum, Dr. H. C. Bumpus, director of the American Museum of Natural History, New York, Dr. N. L. Britton, the director of the New York Botanical Garden, Dr. F. A. Lucas, the curator-in-chief of the Brooklyn Institute, Dr. W J McGee, the director of the St. Louis Public Museum, Dr. W. P. Wilson, the director of the Philadelphia Museums, and Dr. W. J. Holland, the director of the Carnegie Museum. Dr. Samuel Henshaw, the curator of the Cambridge Museum of Comparative Zoology, and Dr. F. J. V. Skiff, of the Field Museum of Natural History in Chicago, were not present, but were represented by letter.

The meeting had been called for the purpose of considering whether it might be advisable to take preliminary steps looking toward the organization in America of a Museums Association somewhat analogous to that which exists in Great Britain. It was finally unanimously decided that the gentlemen represented in this informal gathering should over their names issue a call to the representatives of a number of the larger and more important museums of the United States to convene for the purpose of organizing The Museums Association of America.

In the informal discussion which took place it was decided that the movement should not be restricted to natural history museums, but that museums representing art, as well as the sciences, should be included in the call, and that the invitations should be made to cover the institutions of America, using the word in its widest sense, so as to include all of North America and South America and the various insular possessions of the United States and Great Britain in the western hemisphere.

In accordance with a resolution adopted invitations to attend a preliminary gathering for the purpose of organizing The Museums Association of America will shortly be issued to a number of institutions and individuals who are thought to be likely to be interested in such a movement. This conference will be held at the American Museum of Natural History in New York on May 15, 1906.

PITTSBURGH, PA.,
December 23, 1905.

SCIENTIFIC NOTES AND NEWS.

THE New Orleans meeting of the American Association for the Advancement of Science and the societies affiliated with it begins today. This evening Professor W. G. Farlow, of Harvard University, will give the presidential address, his subject being 'The Popular Conception of the Scientific Man at the Present Day.' We hope to publish this address next week.

THE Paris Academy of Sciences has awarded the Lalande prize to Professor William Henry Pickering, of Harvard University, for his discovery of the ninth and tenth satellites of Saturn.

Dr. William H. Welch, professor of pathology in the Johns Hopkins University, has been elected a trustee of the Carnegie Institution of Washington, to fill the vacancy caused by the death of John Hay.

Professor F. E. Lloyd, of Teachers College, Columbia University, has accepted the position of resident investigator at the Desert Botanical Laboratory of the Carnegie Institution, at Tucson, Arizona.

Dr. Pehr Olsson-Seffer, for the past two years instructor in botany in Stanford University, has been selected to take charge of a botanical station in tropical Mexico for the investigation of problems connected with the cultivation of rubber and coffee. The station is established by the Hidalgo and La Zacualpa Companies, which own twelve large plantations in the region in question devoted to the raising of rubber and coffee.

Professor Harold Heath, associate professor of zoology in Stanford University, will be absent from the university during the coming semester on sabbatical leave. He will occupy the Smithsonian table at the Naples Aquarium for two months, visiting Egypt, India and spending two months in Japan, returning to take up his work September, 1906.

Dr. L. Radlkofer, professor of botany at Munich, has celebrated the fiftieth anniversary of his doctorate.

Sir William Thiselton-Dyer, whose resignation of the post of director of the Royal Botanic Gardens at Kew we announced last week has held that appointment since 1885, and for ten years—1875–1885—before his promotion he was assistant director. His successor, Lieutenant-Colonel David Prain, had a distinguished university career at Aberdeen and Edinburgh before he entered the Indian Medical Service in 1884. Three years after his arrival in India he was nominated curator of the Calcutta Herbarium; in 1895 he became professor of botany at the Medical College, Calcutta, and superintendent of the Royal Botanic Garden there, and in 1898 he was appointed director of the Botanical Survey of India. He is forty-eight years of age.

Dr. Richard Hodgson, secretary of the American Branch of the American Society for Psychical Research, died suddenly at Boston on December 20.

Professor Heinrich Meininger, formerly professor of technical physics, at Karlsruhe, has died at the age of eighty-four years.

We are requested to state that owing to a strike in the printing office at Easton, Pa., the issue of the December number of the *Journal of the American Chemical Society* has been delayed, and the issue of that journal and of the *American Chemical Journal* for January will also be delayed.

The engineering students of Washington and Lee University met on December 4 for the purpose of organizing a local scientific society. Addresses were made by Professors Stevens, Humphreys, Campbell and Howe.

The council of the Iron and Steel Institute has arranged that the annual general meeting of the institute shall be held in London on May 10 and 11, 1906. In place of the usual autumn meeting, a joint meeting with the American Institute of Mining Engineers will be held in London on July 23 to 28. It is intended during the week following to give the American visitors an opportunity of seeing some of the ironmaking districts. It is anticipated that the visiting party will include many of the leading ironmasters who entertained the Iron and Steel Institute in America in 1890 and 1904. The Lord Mayor has consented to act as chairman of the London reception committee, and to give an evening reception at the Mansion-house.

The sixth International Congress of Criminal Anthropology will open at Turin on April 28, 1906. The following questions are proposed for discussion, and the communications presented will, as far as possible, be grouped round these as central themes: (1) the treatment of juvenile criminality according to the principles of criminal anthropology, to be introduced by M. von Hamel; (2) the treatment of female criminality, to be introduced by Dr. Pauline Tarnowsky; (3) the relations of economic conditions to criminality, to be introduced by Professor Kurella; (4) the

equivalence of the various forms of sexual psychopathies and criminality, to be introduced by Professor C. Lombroso; (5) criminal anthropology in police organization, to be introduced by Professor Ottolenghi; (6) the psychological value of evidence, to be introduced by Dr. Brusa; (7) prophylaxis and treatment of crime, to be introduced by Dr. Ferri; (8) establishments for the perpetual detention of criminals declared to be irresponsible on account of mental defect, to be introduced by Professor Garofalo.

THE New Haven correspondent of the N. Y. *Evening Post* states that a collection of Central American antiquities, the value of which was not suspected, has just been brought to light in the Peabody Museum and, when arranged, will be put on exhibition in the anthropological department. Beginning as early as 1860, A. De Zeltner, French consul at Panama, and Mr. J. E. McNeil, for many years a resident of Panama, collected in that province antiquities of the Chiriqui Indians, who ranked next in culture to the Aztecs and to the Peruvians under the Incas. The collections, chiefly secured from prehistoric graves, were brought down from the interior on horses, but, as the result shows, with only slight breakages. The late Professor O. C. Marsh bought the collections from time to time, and down to the year 1879, storing the boxes away in a remote part of the museum building, where they have remained unopened for twenty-six years.

MR. CHARLES S. SPANG, formerly of Pittsburg, who recently died in Paris, where he spent the latter half of his long life, before his death requested his heirs to turn over to the Carnegie Museum his collection of Etruscan pottery and Egyptian antiquities. In accordance with his wish these collections have come into the custody of the museum. The collections were made nearly fifty years ago by a gentleman whom Mr. Spang employed to make excavations, and the specimens are remarkably fine and such as could not well be obtained to-day.

DR. ZAMBACO PACHA has devoted the sum of 10,000 francs towards the organization of a medical congress to be held every three years at Athens.

THE liabilities of the Royal Botanical Society of London now exceed £30,000. It is proposed to increase the dues from two to three guineas.

AN Association of Municipal Engineers, Architects and Hygienists of France, Belgium and Switzerland held its first meeting in Paris from November 22 to 25.

THE New York Academy of Medicine announces that the Edward N. Gibbs memorial prize, of the value of $1,000, will be awarded to the author of the best essay on 'The Etiology, Pathology and Treatment of Diseases of the Kidney.' Essays must be received on or before January 1, 1907, by the recording secretary at 17 West Forty-third Street, New York City.

THE Senckenburg Natural History Society at Frankfort offers a prize of 1,000 Marks for a research on the paleontology of the region about Coblentz and Ems.

THE *British Medical Journal* states that the trustees of the Pilkington Cancer Research Fund are about to appoint a graduate in medicine, or other qualified person, to carry out a research into the cause, prevention and cure of cancer, under the supervision of the professors of general pathology and of systematic surgery in the Victoria University of Manchester (Professor Lorrain Smith and Professor G. A. Wright). The appointment will be for one year, but may be renewed for a further period of one or two years, and the holder of the post will receive an income of £300 per annum with a grant for laboratory expenses.

ACCORDING to *Nature* an archeological museum, which will devote special attention to Indo-Chinese matters, has been established by the French government at Pnompenh. The museum will be under the scientific control of the Ecole française d'Extrême-Orient, the chief of the archeological department of which school will act as director of the new museum.

ACCORDING to the statistics collected by Mr. Waldemar Lindgren for the U. S. Geological

Survey, the production of gold in the United States during 1904 amounted to 3,910,729 fine ounces, valued at $80,835,648. This represents an increase of $7,243,948 over the production of 1903. After a period of very rapid advance in the gold production from 1892 to 1900, during which an increase from $33,000,000 to $79,171,000 took place, there followed two years of nearly stationary output and one year, 1903, of very decided decrease. It is, therefore, very gratifying to find that the production has risen again with a bound to record figures, the largest previous output in 1902, amounting to $80,000,000. The production of silver in 1904 amounted to 55,999,864 fine ounces, valued at $32,035,378. This represents an increase of 1,699,864 ounces over the production of 1903, and an increase in value of $2,713,378. There is, therefore, a total increase of $9,957,326 in the value of gold and silver produced in 1904 over that of 1903. The record output of silver in 1892, amounting to 63,500,000 fine ounces, has not been reached in late years, nor has the commercial value attained the figures of that year, which amounted to $82,101,000. The price of silver in 1904, according to the director of the mint, varied from 55 to 61 cents per fine ounce, representing a decided increase over the prices of 1903, which varied from 48 to 59 cents and only exceptionally rose to 61 cents in October, 1903.

UNIVERSITY AND EDUCATIONAL NEWS.

The council of Columbia University has adopted resolutions as follows:

Resolved, (1) That in the opinion of the University council the present game of football should be prohibited at Columbia University and the council recommends that the president take immediate action to that end.

(2) That the president be advised to take such further steps as may seem to him proper to correct the conditions at Columbia, which have produced the demoralization of sentiment above referred to and to restore the athletics to their proper place in the life of the university, with the view,

(*a*) Of encouraging the widest possible participation of the student body in athletic sports, instead of leaving them, as at present, in the hands of a small class of trained athletes.

(*b*) Of substituting, as far as possible, competition in sports among the students at Columbia in the place of intercollegiate competitions, and of restricting the latter, with the exception of rowing, as far as possible, to the home grounds of the Columbia teams.

(*c*) Of eliminating the professional aspects of athletic sports by reducing to a minimum the time devoted to training and by placing the sports and the training therefor under the immediate direction of the university authorities.

(*d*) Of suppressing the commercialism attendant on intercollegiate competitions in certain sports by a radical reduction of expenses and the elimination of gate receipts.

At a meeting of the presidents of the two California universities with the committee on athletics of each institution held recently in San Francisco the following action was taken:

Resolved, by the joint athletic committees of the University of California and the Leland Stanford Junior University, that we recommend to the faculties of the two universities in question that the intercollegiate football contest shall be no longer held under the regulations of the present football rules committee. We recommend as a substitute the present English Rugby game, or else the present American game with such modifications as shall promise to eliminate the existing evils.

A committee of men prominent as coaches and players of the game from among the alumni of the two institutions was selected to work with the faculty committees in framing a final decision in the matter.

Dr. Henry T. Eddy, professor of engineering and mechanics in the University of Minnesota, has been elected dean of the Graduate School at that university; and Dr. George F. James, professor of pedagogy, has been elected dean of the School of Education, just established.

Professor Simon J. McLean, for three years associate professor of economics and acting head of the department in Stanford University, has accepted a call to a chair in his *alma mater*, Toronto University, and will take up his duties there in January.

Jas. R. Withrow, Ph.D. (Pennsylvania), has accepted an instructorship in chemistry in the University of Illinois.

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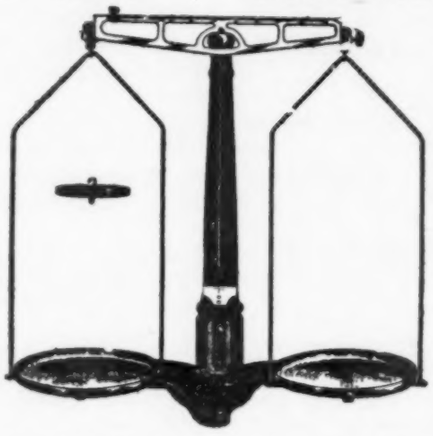

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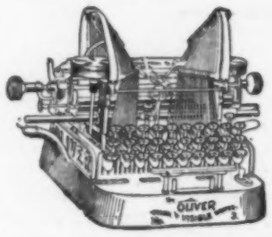

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